DOE'S COMPREHENSIVE PROGRAM MANAGEMENT PLAN FOR MAGNETIC FUSION ENERGY

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

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DOE'S COMPREHENSIVE PROGRAM MANAGEMENT PLAN FOR MAGNETIC FUSION ENERGY

THURSDAY, OCTOBER 20, 1983

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION,
Washington, D.C.

The subcommittee met, pursuant to call, at 9 a.m., in room 2325, Rayburn House Office Building, Hon. Marilyn Lloyd (chairman of the subcommittee) presiding.

Mrs. LLOYD. The subcommittee will come to order.

Good morning, ladies and gentlemen. Today we are going to receive testimony on the DOE comprehensive program management plan for the magnetic fusion energy program.

The Magnetic Fusion Energy Engineering Act of 1980, originating in this committee, expressed the intent of the 96th Congress in terms of a timetable and related funding to reach the goal of commercialization of fusion energy. Unfortunately, the 1980 act could not have been passed in the last Congress or in this Congress because of budgetary pressures resulting from both soaring deficits and certain redefinitions of the Federal role in research and development. Today we shall return to the question of how best to accelerate the fusion program under more restricted funding scenarios. I would note, however, that the committee still believes that modest enhancements above flat funding could provide considerable program momentum in the near term.

As managers of public funds, we are accountable for the organization and the conduct of research and development work in the most effective and prudent manner. In this sense, I view the comprehensive program management plan as a useful starting document which presumably will continually evolve in both objectives and strategy.

The detailed adequacy of the June 1983 plan submitted should not be nearly so great a concern to us as the broad outlines of the plan. I believe the major differences among the administration, the fusion community and the Congress are not very significant and some modifications can be made to accommodate a real concensus on the proper thrusts. This should happen as the program evolves in response to opportunities such as the TFCX initiative and the upgrade for TFTR, which we expect to hear more about today.

I am interested in whether our non-DOE witnesses generally believe that proper objectives and major milestones have been set for the magnetic fusion research program. My impression is that we

must stay flexible enough so that we can ultimately meet our goal on the development of magnetic fusion as a viable energy alternative on a realistic, but at the same time, opportunistic timetable.

Today we will hear from the DOE, its own magnetic fusion advisory group, our subcommittee advisory panel, and fusion power associates. We appreciate this advice and certainly we enjoy seeing several familiar faces and hope that we can synthesize these different views in a way that is useful to both the Congress and the administration. The testimony appears very constructive to me and I am anxious to discuss the various recommendations before us.

Certainly I do want to welcome our first witness. He has been a friend of ours for a long time. He is an outstanding civil servant, Dr. John Clarke, who is Associate Director of the Office of Fusion Energy in the Department of Energy.

Good morning, John. It's nice to see you. We have your testimony and you may proceed as you wish. But your complete testimony will be made a part of the record.

I might add at this time that Mr. Walker will be here. They are having, I think, a Republican meeting. But he will have a statement and he will be here shortly.

Please proceed.

[The biographical sketch of Dr. Clarke follows:]

U.S. Department of Energy
Assistant Secretary for
Congressional, Intergovernmental
and Public Affairs
Office of Public Affairs
Washington, D.C. 20585
202 252 5644

BIOGRAPHY

DR. JOHN F. CLARKE

Dr. John F. Clarke was appointed Associate Director for Fusion Energy, U.S. Department of Energy on August 31, 1982. He had served as Deputy Associate Director for Fusion Energy in DOE's Office of Energy Research since 1979. Before that, he was Deputy Director for the Office of Fusion Energy, assuming that post when DOE was established in 1977. In his present position, Dr. Clarke has responsibility for the federal magnetic fusion program.

Dr. Clarke came to DOE from the Oak Ridge National Laboratory (ORNL) where he had served since 1966. At Oak Ridge he worked on magnetic mirror research until 1968 when he helped found the tokamak program. He was responsible for diagnostic instrumentation and theoretical interpretation of results from the first U.S. tokamak, ORMAK. In 1974, he was appointed Director of the ORNL Thermonuclear Division, assuming responsibility for all fusion research at Oak Ridge. During that period, he made numerous contributions in fusion related plasma physics, established a comprehensive fusion technology program, and strengthened the close coupling to the plasma physics experimental program. During this time he was influential in the development of modern toroidal reactor designs, based on new scientific insights.

In 1961, Dr. Clarke received a B.S. degree with majors in physics and philosophy from Fordham University. He was awarded a master's degree in physics from the Massachusetts Institute of Technology (MIT) in 1964 and in 1966, a Ph.D. degree in nuclear engineering, also from MIT. His doctoral thesis related to confinement of particles in magnetic mirror devices, then the basic fusion confinement system.

Dr. Clarke is the recipient of numerous honors and awards, including the DOE Exceptional Service Medal for scientific leadership and achievement in 1979, and the Distinguished Associate Award from the U.S. Energy Research and Development Administration in 1977. In 1981, he completed a special study for the Secretary of Energy on the adequacy of technical personnel for the U.S. nuclear program. He has represented the United States at a number of international scientific conferences and in negotiation of international fusion agreements.

Dr. Clarke was born in Hempstead, New York.

October 1982

STATEMENT OF JOHN F. CLARKE, ASSOCIATE DIRECTOR, OFFICE OF FUSION ENERGY, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. CLARKE. Thank you, Madam Chairman.

You do have my formal testimony. In the time that I have available to me, I think I would just like to make a few general remarks which will paraphrase some of the key points in that formal testimony.

My remarks today are very much in line, I believe, with your opening remarks, in that I'm going to treat the comprehensive program management plan as a general outline of the Department's policy for fusion. It really provides us with a framework within which we can consider more detailed plans that we're developing or the subprograms within the overall magnetic fusion program (fig. 1).

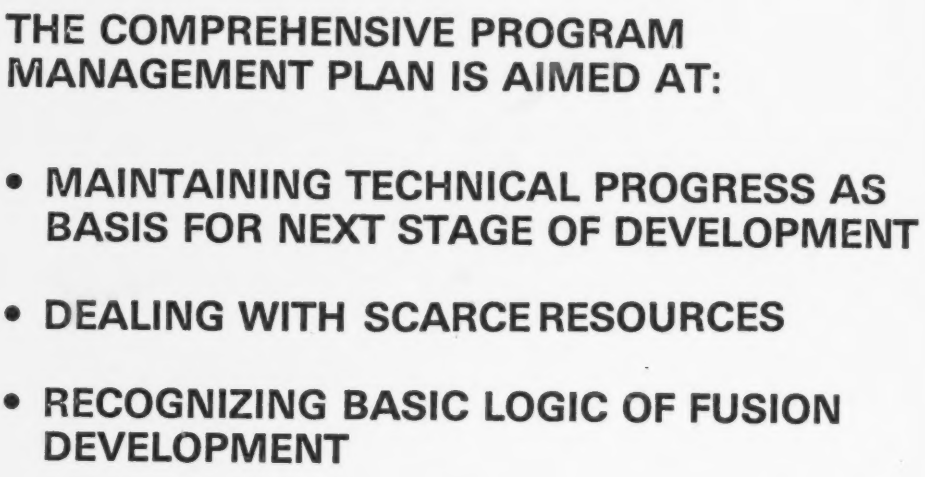

THE COMPREHENSIVE PROGRAM MANAGEMENT PLAN IS AIMED AT:
• MAINTAINING TECHNICAL PROGRESS AS BASIS FOR NEXT STAGE OF DEVELOPMENT
• DEALING WITH SCARCE RESOURCES
• RECOGNIZING BASIC LOGIC OF FUSION DEVELOPMENT

FIGURE 1

The first thing I would like to deal with is to tell you about the motivations and some of the underlying reasons why the comprehensive program management plan was prepared as it was. We were trying to accomplish three things in this plan. We were trying to put together a plan which maintained technical progress in the fusion program because that technical progress is a necessary basis for the next stage in fusion development. Second, we were trying to deal with the reality of scarce resources to which you referred. Finally, we were trying to put together a plan which, in the face of scarce resources, still contained all of the basic elements that have to be included in a comprehensive fusion program.

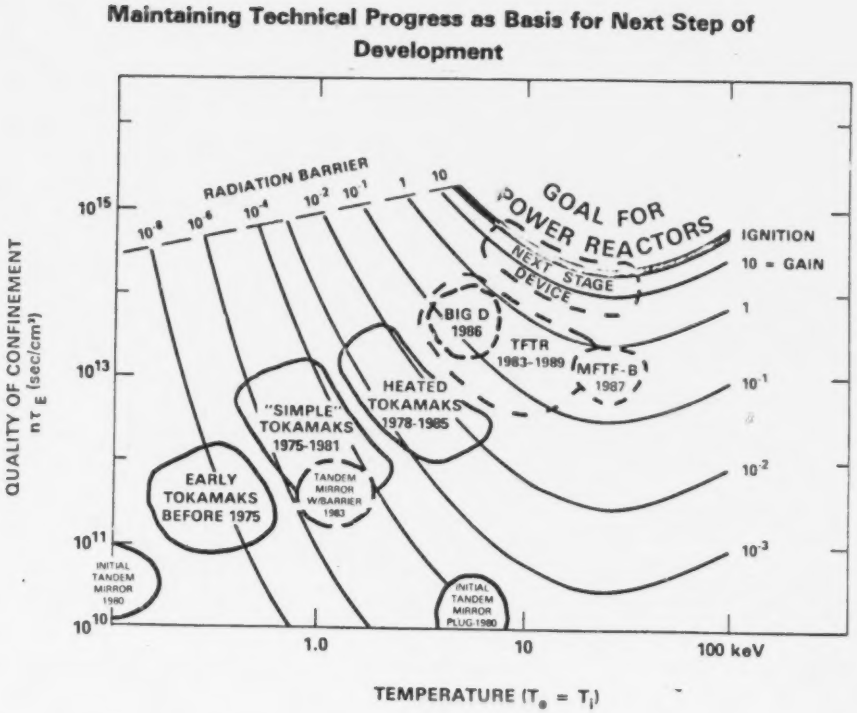


FIGURE 2

The first thing that I would like to deal with then is this question of technical progress (fig. 2). I believe on the vugraph you can see the familiar chart that we show you to plot our progress in fusion. On the vertical axis it shows the quality of confinement that we've been able to achieve in our fusion devices, experiments, and on the horizontal axis you can see the temperature attained.

The point I want to make this morning, aside from emphasizing the continual progress the program has made over the last decade, is the fact that the present generation of devices described in the comprehensive program management plan—as shown here, the large Dee at General Atomic, the TFTR at Princeton, and the MFTF-B at Livermore—are the devices that will be exploring the physics of plasmas at the verge of the power reactor regime. As a result, I think it's obvious that the next stage device in the fusion program will have to be a device that enters that reactor plasma regime. I think that's a general and obvious fact that is accepted by the scientific community.

So in our preparation of the comprehensive program management plan we were most concerned with preserving the program, the people, the facilities that have brought us the progress to date, and with preserving the progress toward this present generation of facilities, the big Dee, the TFTR and the MFTF–B, so as to prepare the basis for the next logical scientific step.

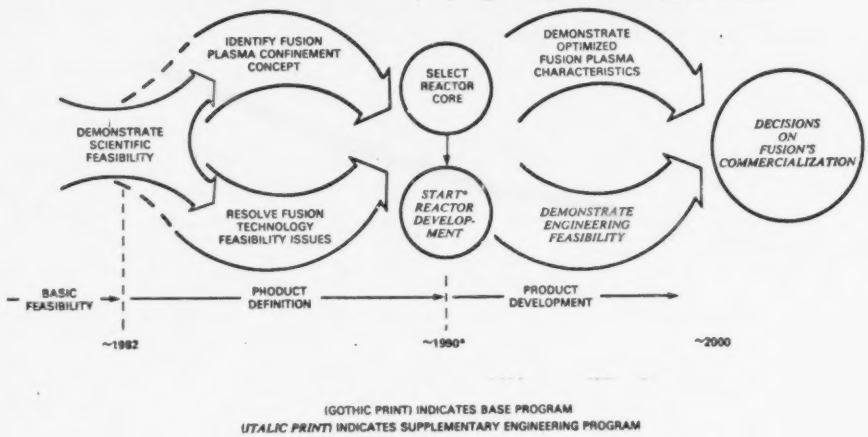
ANY FUSION PROGRAM PLAN MUST:

- ACHIEVE SCIENTIFIC FEASIBILITY
- PREPARE A SCIENTIFIC BASE
- DEVELOP A TECHNOLOGY BASE
- REDUCE SCIENCE AND TECHNOLOGY TO PRACTICE THROUGH ENGINEERING DEVELOPMENT

FIGURE 3

Now, beyond that, we had to deal with scarce resources and the logic of fusion development. Any fusion program plan must have within it the elements which can achieve scientific feasibility, which can prepare a scientific base, which can develop a technology base, and finally, which can reduce the science and technology to practice through engineering development (fig. 3). Now, our job in preparing this plan was to try to incorporate these basic elements of any fusion plan into a form which was compatible with the present resources available to the program. That has led us to the arrangement of the program logic as shown in the comprehensive program management plan.

CPMP
DEALS WITH SCARCE RESOURCES AND FOLLOWS BASIC LOGIC OF FUSION DEVELOPMENT



(GOTHIC PRINT) INDICATES BASE PROGRAM
(ITALIC PRINT) INDICATES SUPPLEMENTARY ENGINEERING PROGRAM

FIGURE 4

In the next stage of the program, because of the scarce resources, we must do three things (fig. 4). We must finish up our demonstration of scientific feasibility on the TFTR facility; we must resolve the technological feasibility issues that underlie both the next steps in the program and the ultimate usefulness of fusion; and again, because of the scarce resources, we feel that we must make a selection in the program within the next decade to allow us to concentrate our efforts on one line of development.

This is a very serious step in the program because historically we have tried to maintain as much breadth as possible within the fusion program, but given the resources available, this is an inevitable step in the program. [Lights out.]

Ah, ha. That may help the audience, but it's not helping me very much. [Laughter.]

[Lights on.]

Mrs. LLOYD. We apologize, but we can't adjust the lighting. Please proceed.

Dr. CLARKE. We are familiar with these technical difficulties.

The other element of the comprehensive program management plan that is driven by the resource availability is the question of engineering development. In our plan we have built in the possibility of an early start on engineering development, but we don't have any definite plans to start that development because it would require additional resources.

TFTR INITIAL ENERGY CONFINEMENT IS ENCOURAGING FOR BREAKEVEN

SIX MONTHS AFTER FIRST PLASMA:

- ONE MILLION AMPERES ACHIEVED (40 PERCENT OF CAPACITY)
- FIFTEEN MILLION DEGREE TEMPERATURE (10 PERCENT OF TARGET) PRODUCES 10^{11} NEUTRONS
- ENERGY CONFINEMENT TIME OF 0.15 SEC (300 PERCENT OF EXPECTED VALUE) SETS WORLD TOKAMAK RECORD

FIGURE 5

One thing I would like to emphasize is that the base program, which is basically the program that we have today, is capable of finishing the TFTR/MFTF-B cycle of experiments and coming to a choice some time in the early 1990's between these two lines of development (fig. 5). Beyond that, the base program is aimed at taking that next logical step in fusion that I referred to before, which is the building and operation of an ignition reactor core device.

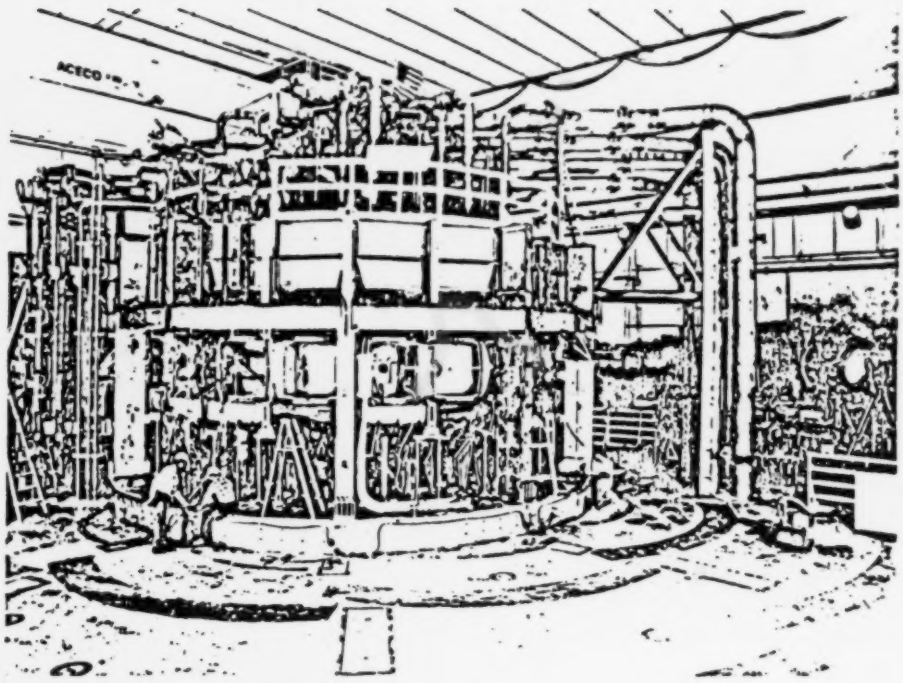


FIGURE 6

Now I would like to turn to some of the subelements in the fusion program. The comprehensive management plan, as I have indicated, is a general framework. Within that general framework we must provide specific subprogram plans with dates and milestones that will mark the progress of the program. One of the major elements in that program, of course, is the TFTR facility—and you have seen this picture before. This vugraph (fig. 6) showing the machine at the end of construction, and I am very pleased to say that the initial operation of this device has, in fact, exceeded our expectations. The machine has achieved a 150 millisecond energy confinement time in its initial preparatory operation. This is very, very encouraging. It gives us great hope that the TFTR will, in fact, meet the goals that we have set out for it in achieving energy breakeven by 1986.

TFTR UPGRADE

- RANGE OF OPTIONS UNDER CONSIDERATION

OPTION	UPGRADE	OBJECTIVE
MINIMAL —	2 SECOND NEUTRAL BEAM ION SOURCES	CONFINEMENT STUDIES
MODEST —	25 MW ICRF, DIAGNOSTICS, SHIELDING, AND REMOTE MAINTENANCE	ALPHA PARTICLE STUDIES

- STATUS
  - ALL OPTIONS UNDER STUDY NOW — NO COMMITMENT

FIGURE 7

Now, given these initial encouraging results on TFTR, and given the need to fully exploit our existing facilities in the late eighties, we have been carrying out studies of possible upgrade options for the TFTR (fig. 7). We have been looking at two different upgrade options; one I might term a minimal option in which we improve the pulse length of the neutral beam sources on the device, and this would allow us to do more extensive confinement studies on that device. A more elaborate upgrade would involve adding up to 25 megawatts of ion cyclotron heating to the device, along with additional diagnostic shielding and remote maintenance capability, and this would allow the TFTR to begin to study the production and dynamics of alpha particles in the device. Alpha particles, I would remind you, are important because it is the addition of heat to the fusion plasma by the alpha particles that is the essence of ignition which underlies the next step in the fusion program.

Now, we haven't yet decided which one of these options would be the best route to take in the latter half of this decade, and we are continuing to study them. But at this——

Mrs. LLOYD. Do you feel with a modest increase in funding we could reach the modest option?

Dr. CLARKE. Well, that's one of the things we're trying to determine right now. As far as the TFTR upgrade is concerned, we think that that's within the base program as it stands now. One of these options will be affordable within the base program.

MFTF-B UPGRADE

OPTIONS UNDER CONSIDERATION:

- INSERT D-T REACTION SECTION IN CENTRAL CELL TO PROVIDE ENVIRONMENT FOR BLANKET TECHNOLOGY TESTING
- UPGRADE END PLUGS TO ENHANCE PHYSICS PERFORMANCE
- COMBINATION OF THE ABOVE TO PROVIDE A FLEXIBLE FACILITY FOR DEVELOPMENT OF NUCLEAR ASPECTS OF FUSION REACTORS AS A COMPLEMENT TO TFCX

STATUS

- UPGRADE DECISIONS WILL DEPEND ON FUTURE ASSESSMENTS OF TANDEM MIRROR DATA BASE AND PROGRAM BENEFIT

FIGURE 8

A second set of upgrade options that we are looking at would be the MFTF-B upgrade (fig. 8). In that case, we are looking at the possibilities of inserting a new central cell into the long MFTF-B central section to provide an environment for nuclear blanket testing. We are also looking at an option to upgrade the performance of the end cells to increase the capability of the device to study physics, and finally, we are considering a combination of these upgrades to both address the physics of tandem mirrors in the reactor regime and also to provide a nuclear testing option.

Now, this upgrade consideration is more long-range than that of the TFTR because we feel that we need an expanded tandem mirror technical base before we would initiate any one of these options. But I think it's very encouraging that we have uncovered a number of very useful potential possibilities for MFTF after it has addressed its initial mission in the program.

FUSION TECHNOLOGY DEVELOPMENT PLAN

SUPPORTS CPMP BY:

- IDENTIFYING CRITICAL TECHNOLOGY PROBLEM AREAS
- ENSURING TIMELY AVAILABILITY OF RELIABLE, MANUFACTURABLE COMPONENTS
- ANALYZING ENVIRONMENTAL AND SAFETY QUESTIONS
- PROVIDING GUIDANCE ON REACTOR ECONOMICS AND COMMERCIAL VIABILITY

MAJOR AREAS INCLUDE

- ENGINEERING DESIGN
- PLASMA TECHNOLOGY
- REACTOR TECHNOLOGY
- FACILITY COORDINATION

FIGURE 9

In addition to these upgrades of major physics facilities, we have been preparing and have issued a fusion technology development plan.[1] I believe this has been distributed to your staff. This plan identifies the critical technology problem areas (fig. 9). It lays out the programs with milestones to address the critical technology areas with regard to engineering design, plasma technology, reactor technology, and facility coordination. I think the technology plan that we've issued is the first of the more detailed subplans within the context of the CPMP and we feel that we will be able to move forward and implement the technology plan within the present level of the program.

[1]Available in committee files.

TFCX STUDIES

- OBJECTIVES INCLUDE:
  - IGNITION, AND
  - LONG PULSE EQUILIBRIUM BURN
- TFCX AS THE OPERATING CORE OF TOKAMAK REACTOR WOULD ACCOMPLISH MUCH NEEDED TECHNOLOGY DEVELOPMENT
- MFAC HAS RECOMMENDED SUCH A FACILITY AS NECESSARY NEXT MAJOR STEP IN TOKAMAK PROGRAM

FIGURE 10

Now, the next future study that I will turn to is something that is farther off within the context of the CPMP. It's the study of the nature of that ignition burning device that I referred to before. It's called the TFCX, the Tokamak fusion core experiment, at the present time. It's true that the name keeps changing, but I think that's in the nature of one of these future studies (fig. 10).

The objectives of this device would include ignition and maintaining an ignited plasma for a long equilibrium burn. That's the reason it's called a fusion core experiment, because the plasma that we would intend to produce in this device is, in fact, the core of a fusion reactor. As such, the TFCX would accomplish the development of much of the technology that's needed to advance from our scientific program into the engineering stage of fusion. The MFAC has recommended such a facility as the next logical step in the fusion program, and I think our comprehensive program management plan recognizes that factor.

In the comprehensive program management plan such a step would be undertaken in the early nineties, after we have made a selection between the tandem mirror and the Tokamak line of development. That decision on timing is driven by the availability of resources.

TFCX STUDY STATUS

- EVALUATION OF ALTERNATIVES UNDERWAY.
- PPPL TAKING LEAD WITH SUPPORT FROM OTHER LABORATORIES AND FEDC.
- INDUSTRY INVOLVEMENT PLANNED IN CONCEPTUAL DESIGN.

FIGURE 11

The present status of the studies (fig. 11) is that we have identified a number of very attractive alternatives for the TFCX. I say attractive in the sense that the cost of this step in the fusion program has been reduced by nearly a factor of two from previous estimates of what it would cost to proceed with a fusion engineering device. We have given Princeton the lead role in developing the TFCX concept with the advice and support of the other tokamak laboratories, and we are planning to involve industrial design teams later in this evaluation process in order to make sure that we get accurate and reliable cost information on this very vital device.

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INDUSTRY ROLE

- INDUSTRY ROLE WILL BE INCREASINGLY IMPORTANT
- GENERALLY ADDRESSED BY NAS AND THE CPMP
- WILL BE SPECIFICALLY ADDRESSED BY MFAC (CHARGE 7)

  "IT IS NOW TIME FOR DOE TO DEVELOP MORE SPECIFIC PLANS BASED ON THE FINDINGS OF THESE GENERAL STUDIES. WE WOULD LIKE YOUR ADVICE ON A SPECIFIC INDUSTRIAL PROGRAM THAT WOULD BE VALUABLE IN BOTH ACHIEVING CURRENT NEAR-TERM FUSION PROGRAM OBJECTIVES, AND ALSO PREPARING FOR THE ACHIEVEMENT OF LONG-TERM FUSION PROGRAM OBJECTIVES."

- WILL EXAMINE SPECIFIC ISSUES IN BASE PROGRAM AND REACTOR ENGINEERING
  - ISSUES OF COST SHARING, PROJECT MANAGER, TECHNICAL RESPONSIBILITY

FIGURE 12

This brings me, then, to the subject of the industry role in fusion (fig. 12). I know this is a subject of great concern to this committee, and it is, of course, naturally of great concern to us. We feel that the industrial role will be increasingly important as the fusion program progresses, and we have been looking at the subject of exactly how that industrial role should best evolve.

This subject has been addressed by a National Academy of Science panel and also addressed in the CPMP. In the past, the issue has been addressed in a fairly general way. I think we can all agree that industrial involvement is a vital element of future development of fusion if it's ever going to be a practical source of energy, but I think there is a very large range of debate as to exactly what form that industrial involvement should take.

Now, in order to make the discussion a little bit more specific, we have asked our Magnetic Fusion Advisory Committee to address this question of industrial involvement once more. I think the difference between this analysis and previous analyses will be that we are trying to get down to specifics. We are trying to look at issues of cost sharing, the role of industry as project manager, and also the level of technical responsibility and the distribution of this responsibility between industry and the laboratories. So I trust that when that study is completed we will be able to revisit this question of the industrial role in a very constructive manner.

INTERNATIONAL COOPERATION

- NEEDED TO COMPLEMENT U.S. BASE PROGRAM
- ENCOURAGED BY SERIES OF SUMMIT MEETINGS
- REQUIRES SERIOUS CONSIDERATION OF LONG-TERM CONSEQUENCES — NATIONAL RESEARCH COUNCIL STUDY

FIGURE 13

In a similar vein, the question of international cooperation is being reconsidered (fig. 13). We feel that international cooperation is today a vital element of the U.S. fusion program. Faced with limited resources, we have made decisions to depend on certain foreign experiments for certain bits of data. For example, we are depending on the ASDEX experiment in West Germany to provide us with advanced impurity control information, and we are proceeding with multilateral and bilateral cooperative agreements to assure that that information will be freely available to us in the future.

In addition to that, however, there is interest at very high levels in the U.S. Government and in the governments of the industrialized world in enhancing the degree of cooperation in fusion. The series of summit meetings that began at Versailles and was continued at Williamsburg has encouraged the fusion community of the world to seriously consider more elaborate and extensive collaborations, and that is proceeding with a series of planning meetings.

Now, we recognize how serious this enhanced collaboration would be for the future of the fusion program in this country, and in order to make sure that we fully appreciate the consequences of such collaboration, we have asked the National Research Council to put together a very prestigious panel, chaired by Mr. Gavin, of Grumman Corp., to consider the long-term consequences of international cooperation in fusion, to make sure that U.S. interests are recognized early in the process and protect it as we go through this process of cooperation.

SUMMARY

CPMP PROVIDES A USEFUL FRAMEWORK FOR FORMULATING DETAILED SUBPROGRAM PLANS

- PROGRAM FACILITY UPGRADES AND NEXT STEPS
- TECHNOLOGY DEVELOPMENT
- INDUSTRIAL INVOLVEMENT
- INTERNATIONAL COOPERATION

FIGURE 14

Now, to summarize, then, my view of the comprehensive program management plan is that it provides a useful framework within which we can formulate detailed subprogram plans (fig. 14). We need to prepare plans for facility upgrades of facilities that are presently being constructed, as well as the formulation and definition of this next step in the fusion program. We need to consider issues of technology development, industrial involvement, and international cooperation. The comprehensive program management plan provides us with an overall framework within which we can evaluate these various issues and hopefully come out with a coordinated fusion program that recognizes the appropriate balance between these different factors that are part of fusion development.

With that, I think I would like to conclude my oral remarks. Thank you.

[The prepared statement of Dr. Clarke follows:]

Statement of John F. Clarke
Associate Director
Office of Fusion Energy
Office of Energy Research
Department of Energy
Energy Research and Production Subcommittee
of the
House Committee on Science and Technology
October 20, 1983

INTRODUCTION

I am most pleased to have the opportunity once again of discussing with you the latest developments in the fusion program, in particular our Comprehensive Program Management Plan (CPMP) issued last June and the potential upgrades to Tokamak Fusion Test Reactor (TFTR) and Magnetic Fusion Test Facility-B (MFTF-B).

My formal remarks will primarily deal with the nature and rationale for the CPMP. I believe this will be more useful than a paraphrase of its content since the CPMP itself is a very carefully written statement of the Department's long range plans for fusion. It has been thoroughly reviewed within the executive branch and represents our best description of current fusion program planning. The Executive Summary is included as Attachment I.

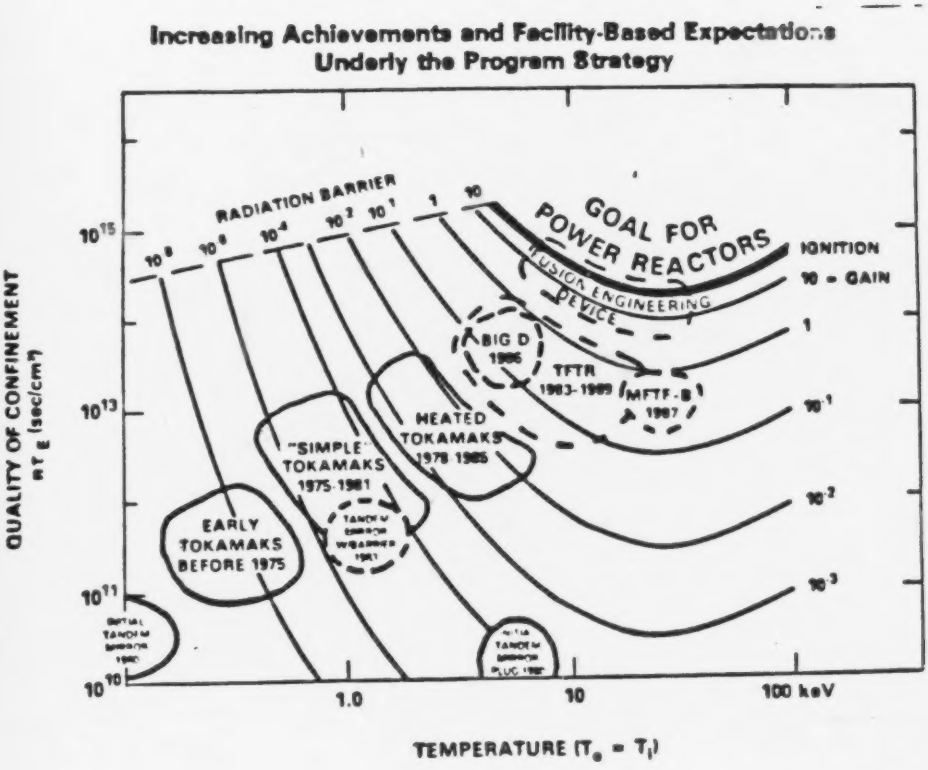
The CPMP constitutes an overall program framework rather than a compilation of detailed subprogram plans. Subprogram plans are continually changing according to the dictates of nature and available funding. The CPMP then is intended to provide a more constant structure of policy assumptions goals and their related priorities. It is needed to ensure that the evolving subprograms constitute a coherent overall program.

Since January, we have examined a number of near-term program options for major facility upgrades. We have also been examining long-term program issues concerning next steps in the fusion program, and we will summarize our thinking on both of these subjects. In addition, we will present the latest fusion office plans in the areas of technology development and our activities to strengthen the role of industry in the fusion program. Finally, we will discuss the potential for international cooperation in major fusion projects.

RATIONALE FOR THE CPMP

The foundation for planning in the fusion program is the technical progress made in understanding the fusion process and in advancing plasma parameters toward reactor-levels. Scientific progress in tokamak and mirror research during the past decade has been great. This progress and the projections for the next decade are shown graphically in Figure 1. It shows the U.S. program's achievements as a function of the two parameters that are necessary for fusion power: quality of confinement (the product of plasma density and plasma confinement time) and plasma temperature. The figure shows clearly that the next step in the scientific program beyond TFTR and MFTF will be into the burning plasma regime characteristic of a reactor. This step in the scientific program will involve, of necessity, a panoply of nuclear and other reactor grade technology and will usher in the age of engineering development for fusion. Maintaining this technical progress in order to advance to this next level of development was the first consideration in our planning.

Figure 1


Increasing Achievements and Facility-Based Expectations
Underly the Program Strategy
QUALITY OF CONFINEMENT
nτ_E (sec/cm³)
TEMPERATURE (T_e = T_i)
RADIATION BARRIER
GOAL FOR
POWER REACTORS
FUSION ENGINEERING
DEVICE
IGNITION
10 - GAIN
1
BIG D
1986
TFTR
1983-1989
MFTF-B
1987
HEATED
TOKAMAKS
1978-1985
"SIMPLE"
TOKAMAKS
1975-1981
EARLY
TOKAMAKS
BEFORE 1975
1.0
10
100 keV

However, technical progress alone was not the only ingredient in preparing the CPMP. A second factor of significance was the level of resources available to the program. Madame Chairperson, I believe you characterized the situation succinctly in the most recent issue of "Energy Business", by noting that the budget climate does not appear favorable for enhanced funding of even successful and desirable programs such as magnetic fusion. The challenge of our planning activities in preparing the CPMP was to design a program to maintain our technical progress toward a worthy goal with efficient use of scarce resources.

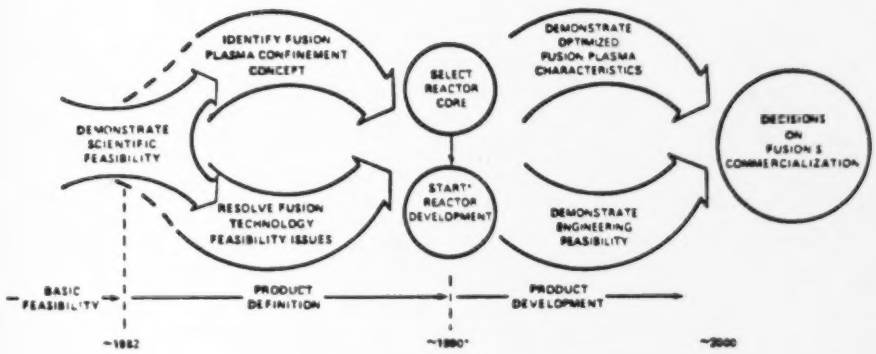
A third factor underlying our plan is that of the fundamental logic of fusion development. It appears that irrespective of available resources, the logical development of fusion must contain four elements:

- Achieving scientific feasibility. This is defined to be a determination that magnetically confined, high temperature plasma can be used to produce a significant amount of potentially useful energy in the laboratory.
- Preparing a scientific base from which a confinement concept with good prospects for a practical application can be selected.
- Developing a technology base from which practical application could proceed.
- Reducing the science and technology base to practice through engineering development.

These logical elements can be arranged in a number of different strategies. The strategy chosen for the CPMP shown in Figure 2 arranges these elements in a program making cost effective use of available resources.

Figure 2

FUSION PROGRAM STRATEGY



- RESOLVE BASIC SCIENTIFIC ISSUES
- IDENTIFY TECHNOLOGY ISSUES
- CONDUCT APPLIED SCIENTIFIC RESEARCH
- DEVELOP TECHNOLOGY BASE
- OPTIMIZE FUSION REACTOR PLASMA
- DEMONSTRATE REACTOR TECHNOLOGY

*TIMING OF START DEPENDS ON TECHNICAL PROGRESS, BUDGETARY CONSTRAINTS AND EXTERNAL PERCEPTION OF NEED

THE FUSION BASE PROGRAM

In defining the near-term components in this strategy, we attempted to determine the minimum program size required to accomplish the various tasks outlined in Figure 2. The base program is defined by the program elements that are needed for demonstration of scientific feasibility, selection and development of a fusion plasma core, and development of technology both to support the science efforts and to explore a limited number of longer term critical technological feasibility issues. These activities are fundamental to any additional engineering development objectives; and, we believe they form an irreducible core if we are to maintain international leadership or even competitiveness for the United States in this frontier area of technology. As defined, this base program can proceed on a fairly steady budget toward the goal of defining an attractive reactor concept for further development.

Once the base program has successfully defined a suitable reactor concept and a technology base, it would then proceed with an effort to optimize the chosen plasma concept and, on that basis, select a set of commercial reactor-level plasma characteristics that appear achievable. The next step in the base program would be a demonstration of these optimized reactor-level plasma characteristics in a specific fusion core experiment based on the selected concept.

In the base program, the two leading confinement approaches, the long-pulse tokamak concept and the tandem mirror concept, will be explored and developed in parallel with associated technology development. Auxiliary tokamak and mirror experiments, and other supporting experiments will focus on providing a scientific base to allow a full evaluation of these two concepts after operation with reactor grade plasmas. A preliminary assessment will be made in 1984 to allow a rebalancing of effort between these concepts and to provide a basis for determination of the facility modifications or upgrades needed to maintain scientific progress through plasma concept optimization in the early 1990's.

The strategy is based on the judgment that the present level of effort provides a technically sound base for continued progress toward a practical reactor in the next century. The annual budget required to implement this strategy and approach on the schedule envisioned above is judged to be at about the current program level, after correcting for inflation, for each of the next five years beginning in fiscal year 1984. This budget level is that required to maintain a comprehensive base fusion program proceeding at a reasonable scientific pace and culminating in a fusion core experiment sometime in the late 1990's. A decision to proceed into the product development phase by starting on an engineering development device would require increasing budgets in subsequent years.

RECENT BASE PROGRAM ACTIVITIES

During the last six months, encouraging results have been obtained from TFTR operations. Also during this period, there have been a number of planning

activities involving upgrades to existing facilities within the scope of the CPMP, and we have issued an integrated plan for development of the fusion technology needed for next steps and reactors. We have also made good progress toward identifying an attractive candidate for the next major program facility, a fusion core experiment based on the tokamak.

TFTR Results

Since first plasma in December 1982, TFTR accomplishments have been excellent. These accomplishments consist of 1 MA of plasma current six months ahead of schedule, highly precise feedback control of plasma position and current, feedback control of density, full computerized control of all systems, and diagnosis of favorable confinement time of 150-180 msec. This confinement time is a world record and is very encouraging as we look to a demonstration of energy breakeven on TFTR in 1986. In addition, during the recent shutdown, all planned hardware additions were accomplished on schedule in preparation for the next phase of operation.

TFTR Upgrade

An upgrade of the present TFTR facility in the late 1980's would allow the United States to play a strong supporting role to the more powerful European JET facility in its tritium operation phase. The objectives of the TFTR upgrade under consideration are to 1) achieve energy multiplication of Q=2, 2) initiate preliminary studies of alpha particle confinement and heating, 3) extend ICRF heating R&D to reactor relevant 10 kev D-T plasmas, and 4) to initiate the development of remote handling for a reactor.

Necessary modifications to the present TFTR machine are being completed. However, additional equipment such as power supplies and heating systems would be required to complete this upgrade. This program would require upgrading the neutral beam sources to at least a three second pulse length, adding 1 or 2 five minute ICRF heating modules, and carrying out a limited remote handling R&D program to provide for the repair of critical elements. The last is a requirement, since Q=2 operation implies activation of the device beyond the limits for hands-on maintenance.

MFTF Upgrade

As noted in the CPMP, we will attempt to maximize utilization of existing fusion facilities. MFTF upgrades will be considered for funding in future fiscal years depending on assessments of the data base for tandem mirrors and on the potential contribution to technical progress.

In conducting studies on the future of MFTF-B, three options have been proposed. They include an option aimed at improving physics performance by upgrading the end plugs; an option aimed at nuclear technology development by upgrading a section of the central cell for DT operation; and an option which combines both these features. If supported by future experiments, these options would make effective use of the large investment in MFTF-B. Depending upon the option selected, they could significantly extend mirror device performance to the equivalent Q=2 range, and/or provide a unique environment for nuclear blanket technology testing with neutron wall loadings of 2 MW/m^2.

Technology Development

As the magnetic fusion energy program moves forward from the scientific feasibility phase into the product definition phase, technological considera tions are becoming as important as scientific ones. Full understanding of the future technology and engineering needs for fusion is critically important and these needs deserve proper emphasis in the planning of the Magnetic Fusion Energy program. In response to these needs, a Fusion Technology Development Plan (FTDP) has recently been published. A summary is included as Attachment II to this statement.

As part of our efforts to ensure comprehensiveness in this planning, a review of the future engineering needs of magnetic fusion was independently conducted by the National Research Council of the National Academy of Sciences in 1982. A Committee on Magnetic Fusion was established to conduct this review which was based among other things on an early draft of our FTDP. Major conclusions of this review were:

1). The technology and engineering needs of magnetic fusion can be identified with a relatively high level of confidence.

2). The future engineering needs of fusion are well on their way to clear definition and none are of such a character so as to preclude solution.

The FTDP provides the details of the technology development program in the context of the Comprehensive Program Management Plan (CPMP). It describes

what is needed to develop the technologies for practical fusion energy systems in both the product definition and the product development phases, although it is far more specific about the former phase.

The FTDP covers virtually all of the technology and engineering activities of OFE and ranging from component development to the integration of an experimental fusion device within the priorities of the CPMP. The first priority is to resolve the critical issues required for continued progress in the program. The second is to resolve long-term issues of fusion. The refinement of existing technologies and the development of other long-term requirements are accorded lower priority under the plan. The FTDP identifies specific objectives in detail and sets forth approaches to these objectives along with time-phased milestones.

Three specific examples of recent significant progress in critical near-term technology development and a longer term feasibility issue are:

1). Two different positive ion neutral beam sources have been operated at the MFTF-B operating parameters of 80 KeV for 30 second pulses producing a world record 96 megajoules of energy in each pulse.

2). A 60 GHz Gyrotron was operated for over 30 minutes at greater than the design power level of 200 kw which is a world record.

3). An in-reactor experiment has been conducted which provided data indicating that total blanket tritium inventory for a fusion plant would be approximately 100 times less than previously calculated. This should greatly improve the environmental acceptability of fusion in the long-term.

FUTURE PROGRAM

Recent planning activities affecting our longer term program deal with the next major step in the technical base program, referred to in the CPMP as the Fusion Core Demonstration Device, as well as the role of industry and international cooperation.

Fusion Core Experiment

As noted above, a Fusion Core Experiment is viewed as the next major step in the technical base program. Although the timing of such a major step is clearly tied to funds availability, it is appropriate to conduct serious planning studies to define what such a step would entail. Our planning studies indicate that if such a step were initiated in this decade, it would be in the form of a Tokamak Fusion Core Experiment (TFCX). As envisioned in the beginning of the planning study, the TFCX objectives include ignition and long pulse ignited burn. These objectives would permit a thorough study of the physics of alpha particle heating and confinement as well as control of the subsequent temperature, density, and current profiles. Also included in the proposed scope of the device is a demonstration of ICRF heating, lower hybrid current start-up at full reactor parameters, long pulse impurity control, and the remote handling capability required for a reactor. This project would be a major step forward for the U.S. and, if undertaken, would complement existing and planned foreign fusion programs.

A limited number of design options which meet the TFCX objectives are now being studied. The Princeton Plasma Physics Laboratory (PPPL) is taking the lead in analyzing the alternatives, with advice and assistance from the

other fusion laboratories and the Fusion Engineering Design Center (FEDC) at Oak Ridge National Laboratory. Current activities are focused on identifying the optimum TFCX concept, with major attention being given to cost minimization. Our plans are to select a reference concept at the end of these preliminary studies and then, if appropriate, conduct conceptual design on that concept.

Industrial Role

The most recent example of DOE's involvement of industry in key activities is the start of a technical support services contract in July. The team, headed by Grumman Aerospace Corporation, will develop common standards and procedures for carrying out design studies of fusion reactors to aid us in directing the program along optimum development lines.

A clear measure of the past efforts at industrial involvement in the magnetic fusion program is the fact that about 30 percent of the FY83 OFE budget was spent in industry directly. However, there is widespread interest in improving the scope and quality of this involvement.

Considerable attention has been addressed to this issue in the Comprehensive Program Management Plan (CPMP), the National Research Council (NRC) report on Future Engineering Needs of Magnetic Fusion, and discussions at ERAB and MFAC meetings. The CPMP indicates the Department's near-term intent to maintain the existing "substantial industrial participation", while the NRC report recommends long-term planning to assure that ". . .a frame-work can be established for the transition from Federal to private financial support in the next century".

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We recognize the need to prepare more specific plans for the role of industry in the fusion program, if we are to successfully make this transition especially in this current period of constrained budgets. Therefore, the Department has asked the Magnetic Fusion Advisory Committee (MFAC) to consider this issue. The initiating "charge" to MFAC states:

> "It is now time for DOE to develop more specific plans based on the findings of these general studies. We would like your advice on a specific industrial program that would be valuable in both achieving current near-term fusion program objectives, and also preparing for the achievement of long-term fusion program objectives.
>
> "Industry has been and will continue to be involved in engineering and in supplying the components needed for fusion development. However, the future role recommended by NRC study requires systematically increasing the intellectual involvement of industry. As the NRC further noted, this must be done with full recognition of the timescale involved in fusion development. In the past, the program has attempted to achieve this through various mechanisms. The establishment of the Fusion Engineering Design Center, the MFTF-B cost sharing development effort, the procedure followed in the EBT-P program, and recently in the suggestion for industrial participation in the TFCX design process. We would like your evaluation of the benefits and liabilities inherent in these past efforts as well as suggestions for better mechanisms in the future."

International Cooperation

We plan to use international cooperation during the 1980's to complement the U.S. fusion program, and, where feasible, to reduce program costs by sharing the expense of building and operating selected facilities. We are also taking active steps to identify and exploit significant technical opportunities abroad. For example in the scientific area, we have taken advantage of European decisions to build new experimental facilities to obtain important information on particle and impurity control in tokamaks. As an example in

the technology area, a large coil test facility is being constructed in the U.S., but half of the coil complement is being provided by the European Community, Switzerland, and Japan.

Although the U.S. program should remain largely self-sufficient in developing the most advanced confinement concept, the tokamak, we can still benefit from international cooperation in this area. As noted above, major foreign tokamaks will be brought into operation in the mid and late 1980's and their results should help resolve specific tokamak issues not being fully explored here. In addition, international design studies, such as the INTOR workshop series, have already aided in clarifying the program's understanding of key design issues and are expected to continue to do so. Our planning for the next major step in fusion development, the fusion core device, takes full account of foreign capabilities.

International cooperation in fusion is also being affected by the Versailles Summit meeting in 1982 that called for future meetings on international cooperation in various research topics. Such a working meeting on fusion, with participants from the summit nations, was recently held by the Department of Energy. It is our hope that items discussed on possible joint planning at this meeting can lead to important joint participation in major fusion projects in the next decade.

Because of the significance of international cooperation in our program, we have commissioned a study by the National Academy of Science's National Research Council of the use and consequences of international cooperation in the fusion program. We expect that this study will be of great help to us

in understanding the best ways in which to handle international cooperation. We also expect the study to minimize the effects of the intrinsic difficulties in such activities.

SUMMARY

The long-range planning for the magnetic fusion energy program described in the U.S. Department of Energy's Comprehensive Program Management Plan is based on technical progress in the program, realistic funding expectations and the logical elements of fusion development. Since the issuance of the Plan, new and encouraging results have come from TFTR. Also in the past year, a number of options have been developed for upgrading our major facilities, TFTR and MFTF-B to develop our scientific and technical information base further. We have issued and begun to implement a Development and Technology program plan. On the basis of the new tokamak information and highly promising initial studies, we have begun scoping studies to define a fusion core experiment which could be the next major step in the U.S. and world tokamak program.

We have begun an assessment of the role of industry in the fusion program through the MFAC process. High level discussions of international cooperation in fusion have begun.

Again, I appreciate the opportunity to speak to you today on the Magnetic Fusion Energy Program. I would be pleased to answer any questions.

ATTACHMENT

Executive Summary of the Comprehensive Program Management Plan (CPMP) for Magnetic Fusion Energy June 1983

The Federal Government's support of the fusion program is predicated upon the need for the fusion energy option in the Nation's energy future, as well as the near-term benefits associated with developments on the frontier of technology. As a long-term energy option, magnetic fusion energy has characteristics which could allow it to become a principal energy source in the next century. The attractive characteristics include an inexpensive, vast, and secure fuel reserve, as well as environmental and safety features that appear to be desirable. While electricity is an obvious end use for fusion, other possible uses include breeding fissile material for fission plants, generating high grade heat for industrial applications, and the direct production of synthetic fuels.

In the near-term, there is also a benefit from high technology developments flowing from the fusion program. Fusion program development fosters advances in technology areas as diverse as superconducting magnets, high power accelerators and radio frequency systems, advanced materials develoment, and mathematical methods for computers. Fusion research also makes important contributions in advancing electronics, computer control systems, optical measurements and in many other areas.

Policy Summary - The Department of Energy manages the Government's program that seeks to demonstrate the scientific and engineering feasibility of nuclear fusion. The Department's intent is to maintain a leadership role for the United States in the area of magnetic fusion energy research and development. This is being done by conducting a balanced scientific program of research and associated technology development involving various fusion plasma confinement concepts. During the next decade, the Department expects to select one of these plasma confinement concepts for further development as a power reactor core. The Department also plans to develop the magnetic fusion energy technological base and transfer it to industry in preparation for the practical application of fusion.

Strategy - The goal of the Magnetic Fusion Energy Program is to develop the scientific and technological information required to design and construct magnetic fusion power systems. The strategy for achievement of this goal has three sequential program phases: scientific feasibility, product definition and product development.

- Scientific Feasibility - The tasks within the scientific feasibility phase are to resolve basic scientific issues for the purpose of demonstrating scientific feasibility and to identify technology issues for the purpose of planning a minimum cost technology development program.

During the past three decades, the program has focused on the basic scientific and technological issues associated with the production, heating and confinement of high temperature plasmas in various magnetic field configurations. This phase of the program is entering its final period with the start of operations of the Tokamak Fusion Test Reactor (TFTR) facility at the Princeton Plasma Physics Laboratory. TFTR is designed to conduct experiments to achieve energy breakeven, i.e., the generation of as much fusion energy as was required to heat the plasma. The experimental program leading to the demonstration of the scientific feasibility of magnetic fusion energy release, centered around the energy breakeven experiments, is planned for completion by about 1986.

- Product Definition - The idea of a product definition activity is to conduct a science and technology development program from which can result a fusion concept attractive from the viewpoint of practical application. A clear definition of such a concept before extensive engineering development is central to our strategy. There are two general tasks in this phase. The first is to conduct applied scientific research and to develop base technology in order to select a fusion plasma core suitable for practical application. The second is to resolve selected technology issues critical to the subsequent demonstration of engineering feasibility.

We believe that the only realistic candidates for consideration in the 1980's product definition phase are those based upon the technically strongest existing approaches, namely the long-pulse tokamak and the tandem mirror. These two confinement concept development programs are competitive but they have not evolved in the same way nor are they yet equal in achievement. In the competition between these concepts, we will strive for significant improvements, such as higher efficiency, simpler designs and smaller size. As in the past, we will provide stimulation for this improvement process by supporting research on alternative approaches to the solution of major problems. Selection between the two leading concepts is planned after both have explored reactor-level plasma performance in the program's currently approved facilities; i.e., TFTR, Doublet-III-D, Mirror Fusion Test Facility-B (MFTF-B), and the smaller supporting facilities. This selection process would allow effective use of program resources through focused development of the most promising confinement concept in the future. It will also provide a confinement concept choice for the power producing core of an engineering development device.

The design of such a device will be initiated during this phase in preparation for a subsequent reactor engineering program. The engineering program, based on the selected concept, is intended to demonstrate the engineering feasibility of fusion. Reference preconceptual designs for the engineering device or devices will be maintained and periodically updated during this phase to incorporate pertinent advances.

The deliverable result of this phase is planned to be the definition of the scientific and technological characteristics of a fusion reactor and a realistic estimate of its cost.

- Product Development - With the selection of a fusion plasma confinement concept planned for the end of the decade, the third phase, product development, could begin. The tasks in this phase would be to optimize the selected fusion reactor plasma concept and to demonstrate the necessary reactor technology in order to prove engineering feasibility. This approach is expected to provide the foundation of scientific and engineering knowledge for private sector decisions on fusion's commercial application.

Program Implementation - This strategy will be carried out through a base and an incremental program. We have identified tasks to be accomplished in each.

The base program is designed to accomplish the various tasks in the product definition phase. Those tasks are: selection and development of a fusion reactor core, development of the technology to support the science efforts and the development of a limited number of longer term critical technology feasibility issues. The activities required to complete these tasks form an irreducible core for maintaining an international leadership role for the United States in this frontier area of technology. As defined, the base program can proceed on a fairly steady budget toward a long term goal of readiness for commercialization decisions.

In addition, an incremental reactor engineering program will be required, ultimately, to determine the practicality of fusion power. This engineering program could be initiated during any one of the three sequential phases to accelerate the translation of the technical information base into a practical application of fusion energy. That program initiation will depend upon the country's perception that accelerated fusion development is desirable.

In our strategy, the increased effort required for such a reactor engineering program would be applied with a high degree of certainty to the specific needs of the engineering feasibility demonstration, once the base program has successfully defined a suitable reactor concept and a technology base.

This set of base and reactor engineering programs could result in readiness for private sector decisions on commercial applications in the United States around the turn of the century.

Concept Improvement - An important element of the base program is research on diverse physics concepts that offer significant operating improvements for the mainlines as well as those distinctly different in confinement principles from the mainlines. In either case, this research provides challenge and stimulation to the mainlines through exploration of novel approaches to common problems and will improve the technical base of the program. Some confinement improvement schemes may even have the potential to develop into a product which might be attractive enough to supplant a mainline concept fusion reactor.

International Leadership and Cooperation - The United States has attained a leadership role in world-wide fusion activities through the conduct of a program in which a climate of innovation in science and technology is maintained within a balanced, comprehensive, and integrated R&D program.

The Department intends to maintain this position in the two major confinement concepts and in the development of critical technologies. We recognize that progress can best be made through a carefully formulated and managed policy of close international cooperation to share specific tasks.

Industrial Participation - We plan to maintain the substantial industrial participation already in place in the next phases of the program. Although the industrial participation takes many forms, from supplying hardware to performing development work, it is especially important in the area of systems studies work because of the breadth and potential for valuable information transfer to and from industrial personnel. The system studies work to develop conceptual reactor designs for the leading concepts, as well as system studies necessary to prepare for the selection between long-pulse tokamaks and tandem mirrors, will be performed as a joint effort of national laboratory teams and the Fusion Engineering Design Center (FEDC), which is an industrially based, central engineering design resource at the Oak Ridge National Laboratory. The Department will also use an industrial contractor to establish evaluation criteria and normalizing standards, and then assess the design studies against these criteria and standards and review the R&D needs developed in the studies.

The essential elements of the fusion program plan have been presented above. The CPMP document provides a more detailed discussion of the strategy, the implementing plan, and the supporting rationale. Further details can be found in the specific sub-program plans now being prepared. With the program outlined above, the Department is meeting the intent of the Act to pursue fusion energy research and development vigorously to a point where decisions on commercialization can be made around the turn of the century on a firm base of technical information.

I. OVERVIEW OF FUSION TECHNOLOGY DEVELOPMENT

A. Introduction

The Fusion Technology Development Plan (FTDP) contained in this report gives direction to the conduct of the extensive technology research and development program of the Office of Fusion Energy (OFE). This program, in turn, is an element of the overall fusion program policy that was delineated in the March 17, 1983, DOE testimony before the Energy Research and Production Subcommittee of the House Committee on Science and Technology. This testimony is the substantive basis for the preparation by DOE of the Comprehensive Program Management Plan (CPMP) required by the Magnetic Fusion Energy Engineering Act of 1980 (the Act).

The significance of FTDP is that it integrates the diverse technology development activities within the $500-million yearly program of OFE according to a strategy best able to reach the overall fusion program goal, namely, to develop the scientific and technological information required to design and construct magnetic fusion power systems. Clear priorities have been set by FTDP and difficult choices have been made among subprograms to facilitate the development of needed technology and the realization of an advanced engineering facility, such as an Engineering Test Reactor (ETR), a crucial intermediate objective on the way to the fusion program goal.

1. The Fusion Energy Program

Fusion energy refers to energy that is derived from reactions in which nuclei of certain light elements combine and that is produced on a scale comparable to that of the largest electrical generating plants. The fuel for fusion energy is essentially inexhaustible because of the abundance of the light elements. The processes associated with the life cycle operation of a fusion reactor appear more environmentally attractive than those of some of the alternative large scale energy sources. Although the cost of fusion energy cannot be forecast accurately at this early stage of development, there is some likelihood that its cost will fall as technology progresses. In contrast, the costs of alternative sources may rise with fuel scarcity or with measures for environmental greater protection. Thus from the vantage point of today's best knowledge, fusion energy may form an important, even crucial, component of the mix of energy sources for the United States. A similar view is held by the European Community, by Japan, and by the USSR; and these views are backed up by foreign fusion energy programs comparable to the one in the

United States. We can avail ourselves of this attractive option for long-term energy supply through a program of research and development.

The National Energy Policy Plan of July 1981 specifically identifies magnetic fusion energy as one of a select group of technologies that are appropriate for the Federal Government to develop for public use. The government role in magnetic fusion energy is seen as supporter of the high-payoff, but long and costly, development that is needed before economic risks are reduced to a level that private industry can undertake.

The Act calls for the acceleration of the effort of the United States in research, development, and demonstration activities related to magnetic fusion energy. The CPMP shows how the long-term goals of the Act can be accomplished. According to that plan, the primary objectives of the program are designed to make it possible to decide whether to proceed with the commercial development of fusion energy. These objectives are enumerated as follows:

a. Provide the scientific and technological base necessary to select a practical reactor concept and begin engineering development.

b. Operate an engineering device or devices that integrate the components and systems needed for a reactor, demonstrate a power production capability, and, most importantly, demonstrate engineering feasibility.

c. Provide the detailed scientific, engineering, and cost data, as well as the system understanding and evaluation necessary for commercialization decisions.

d. Transfer fusion technology to industry in preparation for the commercial development of magnetic fusion energy.

The program decision process to accomplish these objectives is illustrated in Figure 1.

The CPMP sets forth a general strategy, divided into three sequential phases: scientific feasibility; product definition; and product development. The goal of the product definition phase, to be accomplished in the decade of the 80's, is to identify the range of possible plasma performance that is useful

THE FUSION PROGRAM STRATEGY CONSISTS OF A BASE PROGRAM AND A REACTOR ENGINEERING PROGRAM PURSUED IN THREE SEQUENTIAL PHASES

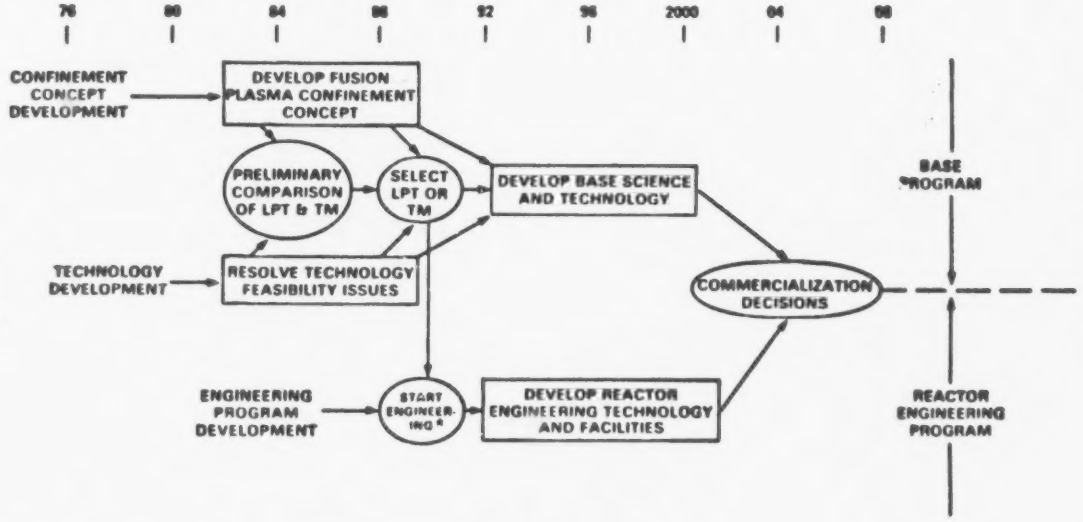

FIGURE 1

PROGRAM PHASES AND TASKS	SCIENTIFIC FEASIBILITY	PRODUCT DEFINITION	PRODUCT DEVELOPMENT
	• RESOLVE BASIC SCIENTIFIC ISSUES	• CONDUCT APPLIED SCIENTIFIC RESEARCH	• OPTIMIZE FUSION REACTOR PLASMA
	• IDENTIFY TECHNOLOGY ISSUES	• DEVELOP TECHNOLOGY BASE	• DEMONSTRATE REACTOR TECHNOLOGY

*TIMING OF START DEPENDS ON TECHNICAL PROGRESS AND EXTERNAL PERCEPTION OF THE NEED FOR PROGRAM COMPLETION. THE REACTOR ENGINEERING PROGRAM COULD PARALLEL OR FOLLOW THE COMPLETION OF THE BASE SCIENCE AND TECHNOLOGY PROGRAM.

for reactor application. The specific objective is to resolve key issues leading to the selection of a confinement concept for the power producing core of an advanced engineering device such as an ETR.

The subsequent product development phase of the CPMP is, of course, less specifically articulated now. In general, however, the goal of this phase is to develop the data base required for construction of commercial fusion power reactors.

2. The Fusion Technology Development Plan

The scope of the FTDP embraces the details of the technology development program in support of the CPMP. The FTDP speaks to both the product definition and the product development phases, although it is far more specific about the former phase. The FTDP describes what is needed to develop the technologies for practical fusion energy systems. As such, the FTDP limits itself to technology matters, without going into policy, management, economic, social, or international issues. The FTDP provides a far greater wealth of detailed technical planning than is done in the CPMP. For example, the FTDP addresses the development of advanced components, the development and evaluation of fusion reactor systems and the materials associated with them, the conceptual design studies of commercial fusion reactors for both electric and non-electric applications, the design and development effort required to prepare for the selection of concepts for and the initiation of an ETR project, the evaluation and control of environmental and safety issues, and the development of standards for all of these areas to ensure the delivery of a high quality product upon the completion of every effort.

The FTDP seeks to advance the objectives of the CPMP, stated above, by pursuing its own related objectives, formulated at a higher level of specificity, namely:

a. Identify critical technology problem areas early enough to permit timely solutions to be developed.

b. Ensure the timely availability of the technology to provide reliable, manufacturable components and systems for incorporation into experimental facilities and eventually into commercial fusion power plants.

c. Evaluate environmental and safety questions and ensure that fusion reactors will operate without undue hazard to operators and the public.

d. Provide guidance to the overall development of fusion so that economic commercial reactor plants can be produced.

The substance of the FTDP extends over virtually all of the technology and engineering activities of OFE and throughout several levels of program complexity, ranging from component development to the integration of experimental fusion devices. Four separate technologies that bear on the generation and maintenance of the high temperature plasma are covered, namely, magnetic field systems, plasma heating techniques, direct energy conversion from plasma particles, and control of particles entering and leaving the plasma.

The FTDP examines six different aspects of those parts of a reactor that surrounds the plasma chamber and are subjected to the flux of energetic neutrons. These so-called reactor technologies are the processing and control of tritium; the design of the first wall, the neutron absorbing blanket around the plasma chamber, and the associated radiation shield; the development of special materials that will perform as desired in the severe reactor environment; reactor components that will withstand especially high heat fluxes; protection against environmental hazards of the reactor; and safety in reactor operation.

The range of technologies embraced by the FTDP runs from the fueling, such as injection into the plasma of neutral particle beams formed from negative ions of extremely high energy, to the mature, such as well engineered magnets of both superconducting and normally conducting coils.

The FTDP sets priorities, the first priority being to resolve the critical issues of technology feasibility and to facilitate the realization of an ETR. The refinement of existing technologies and the development of long term requirements are accorded lower priority under the plan. Figure 2 illustrates these choices.

The FTDP identifies specific objectives in detail in the body of the plan. Also set forth are approaches to the objectives and time-phased milestones along the way. Figure 3 illustrates the more important milestones for the major program elements.

PRIORITIZATION OF TECHNOLOGIES FOR FUSION TECHNOLOGY DEVELOPMENT

First Priority

First priority is placed on technology development issues ETR concept selection (LPT or TMR) in 1988 and critical ETR feasibility issues or fusion power reactor feasibility issues not addressed by ETR

Second Priority[1]

Second priority is placed on the optimization and demonstration of existing technologies, and long term development needs

Concept Selection & Evaluation	Technology Feasibility	

• Reactor Studies
  ○ Improve tokamak and mirror concepts
  ○ Define and prioritize R&D needs
  ○ Define ETR and commerical reactor requirements and criteria for concept selection
 	
• Environment & Safety
  ○ Activities which directly support concept selection or resolution of technology feasibility issues
 	
○ Activities which contribute to selection and narrowing among technology concepts for ETR
	• Blanket and Supporting Materials Development
	  ○ Integrated power conversion and breeding system
	  ○ Breeding materials
	  ○ Structural materials, including low activation and tritium containment
	
	• Maintenance Developments that provide capability for repair or replacement of all components in radioactive environment within acceptable time frame
	
	• High Heat Flux Components & First Wall
	  ○ Special materials & coatings/ particle erosion
	  ○ Limiters, armor, beam dumps (survivability questions)
	  ○ Plasma intractive materials
	  ○ Disruption survival
	
	• Plasma Heating
	  ○ CW negative ion beams 200 kV[2]
	  ○ RF invessel components
	  ○ High power RF current drive components[3]
	  ○ ECRH sources for continuous high frequency, high power
	
	• Particle Control/Impurity Control and Exhaust
	  ○ Tokamak pumped limiters & divertors (continuous pumping & particle control)[3]
	  ○ Mirror end plasma technology (continuous pumping, particle control & direct energy con version)[4]
	  ○ Fueling technology for high density plasmas
	
	• Magnetics
	  ○ Very high field magnets 15T (moderate diameter)[2]
	  ○ Feasibility of large S/C magnetics (LCF, MFTFB)
		Magnetics
		Plasma Heating
		Vacuum Production
		Plasma Position Control
		Diagnostics/I&C
		Power Supplies
		Tritium Processing & Control
		Fueling Devices
		Environment & Safety
		FW/B/S
		Structural Materials
		Alternate Energy Applications
		Alternate Fuel Studies

1 Except as specifically included under First Priority
2 Tandem Mirror Reactor (TMR)
3 Long Pulse Tokamak (LPT)

Figure 2

Fusion Technology Development Program Milestones

Complete Mirror Advanced Reactor Study	1983
Complete proof-of-principle accelerator test for negative ion neutral beam	1984
Complete Generic Environmental Impact Statement on Magnetic Fusion	1984
Complete 90 GHz, 200 KW, C.W. gyrotron development program	1985
Demonstration of 8T Large Coils in LCTF	1986
Complete ETR feasibility technology test for conventional RF heating	1986
Select Prime Candidate Special Purpose Materials for ETR	1987
Complete Conceptual Design of ETR-Tokamak and ETR-Tandem Mirror	1987
Complete preliminary data base for ETR materials	1988
Complete PMI data base for ETR	1988
Complete development of ETR scale fuel injectors	1988
Complete Development of Safety Data Base for Comparative Risk Assessment	1988
Demonstrates 16T Barrier Coil Technology	1988
Operate negative ion neutral beam system on TFTR on MFTF	1989
Complete FW/B/S technology base for ETR	1990
Complete 100 GHz, 1 MW, C.W. gyrotron development program	1991
Complete demonstration of performance of upgraded fuel handling and safety systems in TSTA	1991
Complete proof tests of ETR FW/B/S design	1994

FIGURE 3

Although the FTDP is not a financial plan, Figure 4 gives approximate funding trends for the elements of the plan for fiscal year (FY) 1982 through FY 1988, aggregating about $80 million per year, apart from the cost of major facilities. This amount is consistent with the CPMP assumption that the fusion program budget will remain level during this period. A decision to proceed with an Engineering Test Reactor in FY 1988 would imply increasing budgets in subsequent years.

This budget level will allow the program to address those technology problems that are critical to the design of an advanced engineering device and which involve critical issues for power reactors not addressed by that device. This budget level will not allow, however, all other technology issues to be completely resolved before the completion of this design. Thus the program will take a risk that the advanced engineering device may not reach its planned performance goals.

B. Content of the Fusion Technology Development Plan

No mere overview of fusion technology development can adequately describe the extent of the program performed in dozens of laboratories across the country, nor the sophistication and capabilities of the specialized facilities built to press forward toward new levels of science and engineering. However, within a few pages, the structure of the plan can be indicated. Some idea can be given of the scope of its elements, their concrete objectives, and the most important milestones relevant to ETR design, construction, and operation. The remainder of this section seeks to communicate these aspects of the plan.

1. Engineering Reactor Design and Development

A main focus of the portion of the FTDP devoted to engineering reactor design and development is on the conceptual design for each of the two mainline choices for ETR--the tokamak and the tandem mirror. These conceptual designs are to be completed in 1987. The design and development activity will also produce conceptual design studies for upgrading existing confinement experiments and for the next experimental devices to be constructed following MFTF-B and TFTR.

This element of the plan also calls for conceptual design studies for commercial-scale fusion reactors to help set strategic goals for the entire program and to establish further

FUSION TECHNOLOGY DEVELOPMENT FUNDING
(K$'s, Operating and Equipment Budget Authority)

	FY82	FY83	FY84-88 (in FY84 $'s)
• ER&D			
o Reactor Studies	11700	11000	Increasing by 25% then continuing to increase moderately
o Quality Assurance	0	0	Increasing to $100K then level
o Applications	2000	1500	Decreasing by about 30% then level
• Plasma Technologies			
o Magnetics	20000	19000	Decreasing by about 30% then level
o Heating	20000	22000	Decreasing slightly then level
o Energy Conversion	0	0	Increasing to $100K then level
o Particle Control	1000	1200	Increasing slightly
• Reactor Technologies			
o Tritium	3000	3000	Increasing slightly
o FW/B/S	4400	4300	Increasing slightly with emphasis on tritium breeding
o Materials	17000	18000	Increasing substantially in PMI area and reduced activation materials (other areas level)
o High Heat Flux	800	1000	Increasing substantially (will now include FW elements)
o Environment	1200	1000	Level (includes Engineering Education)
o Safety	1600	2000	Level to slight increase
• D&T TOTAL	82.7	84.0	Increasing by about 10% then level to slight increase

FIGURE 4

research and development needs. Examples are a study of a demonstration tokamak power reactor (DEMO), a commercial tandem mirror advanced reactor study (MARS) to be completed in 1983, and studies updating the STARFIRE and MARS studies of large-scale commercial reactors.

The ETR designs and the commercial-scale power reactor designs together will assist in the selection of the confinement concept that will be developed by the fusion program in the 1990's.

Although non-electric energy applications are accorded second priority under CPMP, nevertheless the FTDP provides for study to assess the feasibility of alternate uses of fusion energy. One such possibility is the use of high-temperature process heat available from a reactor to produce combustible liquid or gaseous fuel--possibly hydrogen or synthetic hydrocarbons. Another possibility is the breeding of fissile fuel using the neutrons available from the fusion reaction.

This element also directs attention to quality assurance and reliability in the fusion energy program. The objective is to provide confidence that facilities, structures, systems, components, and personnel will perform satisfactorily from the aspects of public health and safety, environmental protection, and reliable operation.

2. Plasma Technology

The magnetics element of the plasma technology plan covers superconducting magnet systems that will be needed for fusion reactors. Emphasis is placed on the needs of an ETR. In particular, it is planned to demonstrate satisfactory performance of the 8-tesla (T) toroidal field coils in 1986, using the Large Coil Test Facility. The most stringent magnet requirement in the near term is for 16-T axi-cell barrier coils if the design choice for ETR is the tandem mirror concept. The plan calls for the demonstration in 1988 of 16-T barrier coil technology on MFTF-B using the High Field Test Facility at Lawrence Livermore National Laboratory.

Heating of the plasma to the temperature necessary to initiate fusion may be accomplished by two widely differing technologies. One is the injection into the plasma of energetic neutral particles beams. The other is the absorption of radiofrequency energy. There are great variations of

technique within each technology, and a clear choice of method is not yet possible. Hence, several different techniques need to be pursued to assure successful heating for long pulse, steady state operation in large scale fusion reactors. Several important plasma heating milestones have been adopted: (1) in 1984 a proof-of-principle test for neutral beams formed from accelerated negative ions; (2) in 1985, development of a source of millimeter wave radiation of moderate power, the 200-kilowatt gyrotron tube; (3) in 1986, a test showing feasibility of radiofrequency heating in ETR by the more conventional ion-cyclotron and lower ion-electron hybrid absorption modes; (4) in 1989, operation of a negative ion neutral beam system on TFTR or MFTF-B; and (5) in 1991, a completion of the development of a high power source of millimeter wave energy, the 1-megawatt gyrotron.

Eventually it will be useful to recover electrical energy directly from the charged particles of the plasma exhaust by electric and magnetic conversion systems. Although this topic is provided for in FTDP, it is not critical to ETR operation. Accordingly, it receives a lesser priority under the plan.

The particle control activity is concerned with providing fresh fuel to the plasma and recoming reaction products. An important milestone is the development, by 1988, of ETR-scale fuel injectors using the technique of high speed, solid hydrogen pellets.

3. Reactor Technologies

The Tritium Systems Test Assembly (TSTA) at Los Alamos National Laboratory is the facility that develops the technology required to purify, enrich, contain, control, and dispose of tritium. The objective of this subprogram is to demonstrate the ability to operate a prototypic fusion reactor fuel cycle system reliably and safely. By 1991, there should be a demonstration of the upgraded fuel handling and safety systems at TSTA.

The so-called first wall, blanket, and shield are investigated within a single program element. The function of the first wall is to define the physical boundary of the plasma and to withstand surface heat fluxes deposited by the plasma. The blanket thermalizes fusion neutrons and provides the material within which tritium is produced. The shield attenuates penetrating radiation to levels that allow safe and damage-free reactor operation. Near-term objectives of the first wall/blanket/shield program are to support the selection of the

ETR confinement concept and the associated design effort. Subsequently, the program will lay the foundation for a commercial demonstration. Accordingly, the priority milestones are to complete the technology base for the ETR first wall, blanket and shield by 1990, and to complete proof tests of the corresponding ETR designs by 1994.

Fusion reactor materials constitute a complex and extensive program of first priority. In general the scope of the program is the development of materials and technologies that are required to achieve the objectives stated in the CPMP, including the engineering data for commercial fusion reactors. The materials program element is itself divided into several subprograms dealing with structural materials; materials subject to severe neutron flux; materials in contact with escaping plasma particles; and special purpose materials for such applications as tritium breeding, magnet fabrication, and radiofrequency heating. The objectives of these subprograms are to support the design and construction of ETR and long-term commercial reactors. Certain milestones are important to ETR: (1) by 1987, select prime candidate special purpose materials for ETR; (2) by 1988, complete the preliminary data base for ETR materials; and (3) by 1988, complete the Plasma Materials Interaction data base for ETR.

A further element of the plan covers the development of high heat flux components. These components must be capable of steady-state operation in the environment of a thermonuclear plasma, characterized by severe exposure to high temperatures and energetic particles. These components include for example, first walls, limiters, divertor targets, and neutral beam dumps. The program objective is, as first priority, to develop the technology and the data bases needed for ETR design, and then to transfer the technology to industry for use beyond ETR.

Concern for the environment in the FTDP falls under Reactor Technologies. The associated program element assesses and evaluates the environmental hazards and impacts of magnetic fusion energy development and commercialization. The objective is to see that necessary environmental control technology is developed. An essential milestone in the plan is the preparation in 1984 of a complete Generic Environmental Impact Statement on magnetic fusion.

Similarly, concern for safety in the FTDP falls under the Reactor Technologies. The safety program element evaluates potential

hazards related to the operation of fusion reactors and ensures the development of safety technology and design criteria to control such hazards. The objective is to demonstrate the overall operational safety aspects of fusion through the elimination or control of potential hazards. The plan deals with radioactive inventories, potential plant accidents, design safety and risk assessment, and waste management. A program milestone important to ETR decisions is the completion of a safety data base for a comparative risk assessment for the tokamak and tandem mirror concepts to be done in 1988.

4. Facilities

In addition to laying out a time phased plan for the aspects of engineering development discussed above, the FTDP also briefly describes the facilities within which the technology program is conducted. Many of these facilities are highly specialized, dedicated to particular portions of the diverse fusion energy effort. Examples are the Large Coil Test Facility at Oak Ridge National Laboratory, the Tritium Systems Test Assembly already mentioned, and the Neutral Beam Engineering Test Facility.

Several major fusion devices bring together the technologies and systems originated at the specialized facilities. The principal near term major facilities are TFTR at the Princeton Plasma Physics Laboratory, MFTF-B at Lawrence Livermore National Laboratory.

ETR, of course, is an integrated test reactor that is proposed to be built under CPMP in the 1990's as supported by FTDP. Neither CPMP nor the FTDP schedules a fusion demonstration reactor (DEMO), which would come after the ETR and whose selection would be part of the decision process on the commercial development of fusion energy.

5. Fusion Technology Spin-offs

The FTDP recognizes that the intensive experimentation and technology development conducted in the pursuit of fusion energy may have highly valuable spin-offs that should not go without notice or application. Several areas of likely spin-off are noted, including superconducting magnet technology, high temperature materials technology, high quality vacuum technology, radiofrequency sources of high power and short wavelength, and high energy particle beams. As these technological advances become available through the work of the fusion energy program, they are bound to be adapted to other fields. It is the intent of the FTDP that they be identified and characterized sufficiently well to facilitate that outcome.

Mrs. LLOYD. Thank you very much for your testimony and also for your complete statement, which will appear in the record.

What are the tradeoffs in going beyond TFCX to perhaps more higher payoff or riskier to a more expensive device at this time, John?

Dr. CLARKE. Well, as you know, over the past 10 years, we have looked at a number of different types of future devices for fusion, and these range all the way from elaborate hydrogen experiments with reactor-type plasmas to devices that are little different from working fusion reactors, such as the INTOR design. So we have in hand an array of designs for various machines and we can evaluate then, within this array, the benefits of different types of devices. So I think I can tell you, with a fair degree of accuracy, where the TFCX fits in this array.

It is a device that would contain a burning fusion plasma. The present view is that this is a major scientific hurdle that must be addressed and must be surmounted by the fusion program, and that's the major purpose of this device.

On the other hand, if you're dealing with a burning fusion plasma that would produce several hundred megawatts for 5 seconds, you're perforce dealing with a very serious engineering issue in terms of shielding, in terms of maintaining the device, so that the TFCX would perforce involve a lot of the engineering technologies that we had previously envisioned for the FED or for the INTOR device.

The basic lack, if I can put it that way—I don't want to take away at all from the magnitude of the TFCX when I refer to a lack in this device—is that it would not have a high duty factor and it would not allow for nuclear blanket testing at high duty factors. In terms of getting on with the job and addressing the range of scientific issues that have to be addressed, and in terms of addressing the remote maintenance issues and all of these nuclear technologies associated with the heating and the production of the plasma, I think the TFCX is a monumental step in fusion.

Mrs. LLOYD. Do you think it is logical to continue with this step and not go beyond it?

Dr. CLARKE. You mean to stop with the TFCX?

Mrs. LLOYD. Or to go further.

Dr. CLARKE. Let's see if I understand your question.

Mrs. LLOYD. Do you think the TFCX is a logical step——

Dr. CLARKE. Yes.

Mrs. LLOYD [continuing]. Rather than going on to a more expensive device at this time?

Dr. CLARKE. Yes, I do. I think that—well, there is always a discussion about, you know, how large a step one should take. If we had a situation in which we had a definite date that we could all agree on by which fusion was needed for supplying energy in this country, I think then we could settle that question once and for all. I don't think there is uniform agreement on the date by which fusion has to be available. As a result, I am looking at this next step in the fusion program by rolling forward from our present stage of development.

So my answer to you is basically that the TFCX, as the concept is evolving at this time, represents a very major and necessary step

in fusion. We are trying to define that as a minimum cost step in order to maximize the chances that we'll actually be able to proceed. So there is a deliberate attempt to strip away from the device any of these extraneous features that would be good in the long run, of course, but would also add cost and delay to the device.

As I tried to emphasize, we do have a program in place in this country that has a lot of capability. The scientists in the laboratories are ready—or they feel they will be ready within a few years—to embark on this step in the program. I think the more we delay beyond this earliest possible time, the worse we're off in terms of preserving our present scientific capability. So I would personally tend to go for the minimal device that will represent a major step forward in the program.

Mrs. LLOYD. So we will have something in hand for the dollars spent.

Dr. CLARKE. That's right.

Mrs. LLOYD. What was the response of DOE to the MFAC recommendations?

Dr. CLARKE. We are still considering the latest recommendations. I believe Dr. Davidson will outline those recommendations. I think I had just better not comment on them because we're still considering them.

Mrs. LLOYD. I want to compliment you on the attention you paid to the role of industries. You know, this committee is very concerned and certainly encourages industrial participation in this program.

One thing I would like for you to do, in addition to your complete statement, if you would furnish additional items of work—and we will give you a memo on this—that could be accomplished if the funding could be increased, first by 5 percent and, second, by 15 percent, in fiscal year 1985 from our 1984 levels, if you would just generally characterize for us today what could be done at these levels, or if you would prefer to wait and prepare that for the record.

Dr. CLARKE. I think I would prefer to give that serious thought and provide you with a written statement.

[The material follows:]

Madame Chairman, the base program currently supported by this Administration is an adequate one to meet the objectives of the CPMP and I can assure you that we are addressing the critical issues in the program. Let me address your question by pointing out what the "Fusion Community" believes to be the highest priority items on the margin.

First, there are improvements to the existing base program in specific areas: strengthening the preparations for TFTR operation with D-T, improving selected programs on critical technology issues, and providing additional flexibility to the supporting concepts research program. These improvements would not change the basic characteristics of the program.

Next would come enhancement of the capabilities to accommodate future initiatives in the fusion program such as the Tokamak Fusion Core Experiment. As indicated in my testimony, this experiment would be used to investigate ignition, long pulse equilibrium burn, and the associated technologies.

These areas of research are consistent with those areas discussed by our Magnetic Fusion Advisory Committee and at the recent Energy Research Advisory Board (ERAB) hearing on Magnetic Fusion.

Mrs. LLOYD. Do you generally feel that the management plan will be a useful program guide, then, for evolving objectives and strategies, that it will evolve as we progress?

Dr. CLARKE. I do, and I also feel that it's a flexible plan. As I have indicated, it's got flexibility in terms of when one starts an engineering emphasis in the program. It's also got flexibility in terms of a pacing of the program, and this is solely determined by funding.

As long as the plan indicates that the program can maintain progress at the half-billion dollar level—that is stated as a round number, but a lot of serious analysis has gone into preparing that number—I think as long as the funding is maintained at that level, we will be able to make progress within the framework of the CPMP. The CPMP has also got, as I said, the flexibility for adopting changes as the funding varies, and I think some of the directions of variation are indicated in the plan.

Mrs. LLOYD. In your written statement you mention that 30 percent of the OFE budget was spent in industry——

Dr. CLARKE. In industry.

Mrs. LLOYD. Directly.

How does this work relate to the Fusion Engineering Design Center at Oak Ridge?

Dr. CLARKE. That center is presently funded at approximately $5 million a year. That's a small part of the effort that we expend in industry. Most of the money that is presently spent in industry goes for supplies, buying components and pieces of our apparatus from industry.

The $5 million at the Fusion Engineering Design Center, approximately $5 million, represents a major intellectual involvement of industry, because the people at the Design Center are playing a key role in defining the nature of our future steps, such as the TFCX. So I think in that sense that represents a very special part of our industrial involvement.

Mrs. LLOYD. That's only about 4 percent, though, isn't it?

Dr. CLARKE. That's at the Design Center. The rest of the 30 percent that I referred to is money that is spent in industry buying coils—for example, the large, superconducting coil that Westinghouse is manufacturing for the large coil project. That would be another example.

If you wish, we could provide you with a list of where all of that money is spent——

Mrs. LLOYD. If you would.

[The material follows:]

Madame Chairman, the following list reflects the major industrial participants in the fusion program. It should be noted that there are many more that do participate, but at a much lower level.

Office of Fusion Energy
Industrial Contracts
FY 1982
(Dollars in thousands)

Advanced Environment	$ 139
Airco, Inc.	1,331
Airco Industrial Gases	535
Air Preheater	600
Allis Chalmers	1,900
Alpha Industries	154
American Ind. Tech	214
American Satellite	467
American Science and Engineering	348
Amuneal Manufacturing	207
Applied Microwave Plasma Concepts	200
Austin Research Associates	249
Aydin, Inc.	2,923
Balzers	132
Brown Boveri	607
Burns and Roe	64
Carl N. Swenson	2,139
Chicago Bridge	4,200
Combustion Engineering	1,300
Control Data Corp.	472
Cornell & Underhill	400
Cray Research	660
CVI Corp.	1,826
Devault of Delaware	119
Digital Equipment Co.	3,534
Eastern High Voltage	346
Ebasco/Grumman	3,800
Electro-Engineering	107
Electronic Enclosures	116
Energy Research Associates	209
Everson Electric Co	181
Fiberguide Ind.	155
Ford Construction	318
Foster Engineering	135
GA Technologies	26,262
General Dynamics Corp., Convair Division	16,051
General Electric	2,242
Giffels Associates, Inc	1,000
Gould	1,021
Grand Cal	450
Grinnel Systems	112
Grumman Aerospace	725
Gulton Ind.	157
Henley & Co	160
Hitron Corp.	147
Hughes Aircraft Co.	1,729
Ingersoll-Rand	131

Jaycor	615
Jersey Precast	135
Joerger Enterprises	248
Ken Weld	133
Magnetic Metals Co	141
Martell Construction Co	120
Massachusetts Institute of Technology	313
Mathematical Sciences, NW	725
McDonnell-Douglas Astronautics Co	10,578
Mid-Atlantic	250
Pacific Gas and Electric (PG&E)	260
Peifer	123
Perkin - Elmer	1,600
Pittsburgh - Des Moines	4,531
Raytheon	123
RCA	2,066
Rensselaer Polytechnic Institute	2,215
Rockwell	400
Rotator	200
Science Applications	1,724
Southern Bolt	500
Space Co	229
S. T. Peterson	315
Terminal Construction Co	4,831
Transrex	1,100
TRW, Inc	3,692
Tymnet	108
UCLA	142
Universal Voltro	1,142
Validyne Engineering	137
Varian Associates	2,929
Westinghouse	4,564
Xerox	185

Dr. CLARKE. The largest intellectual involvement, if I can distinguish that from hardware, is in the Design Center activity at Oak Ridge.

Mrs. LLOYD. Since we are very concerned about industrial participation, if you would supply that for the record, we would appreciate it.

I would like to welcome our ranking minority member, Mr. Walker, and Mr. Valentine is with us today.

Mr. Valentine, do you have any questions?

Mr. VALENTINE. Thank you, Madam Chairman.

You mentioned, Doctor, something about international cooperation. Are you satisfied with the state of cooperation that we receive or that you receive in this program from the free world?

Dr. CLARKE. Generally, I think I would say yes. It varies, depending on the country we're dealing with, or specific group of countries. I would single out Japan as a country from which we are getting tremendous benefit in the fusion program. We have very, very constructive interactions with them, and we have developed a very balanced, mutually beneficial arrangement with them.

Mr. VALENTINE. Are there any joint projects to which this country and other countries work on together, and if so, are those projects funded disproportionately by this Nation, or is the cost of those operations shared fairly equally?

Dr. CLARKE. I think the precise answer to the question is that there is not an equal sharing at all projects. We have tried to formulate overall collaborative agreements with countries so that within the overall agreement we get a net balance. I can give you an example of that.

In our relations with Japan, for example, we have constructed a very large test facility at Oak Ridge and we are providing three test coils plus the facility and Japan is providing one coil, and we are sharing information equally. So in that sense we are providing more than we're getting.

On the other hand, in the Doublett III facility at General Atomic, the Japanese Government has provided over $60 million to the United States to operate that facility, and they have provided us with a very skilled team of physicists to operate one of the shifts of that device. In that case, the United States is clearly getting a benefit from Japan.

Overall, we make an attempt to try to make these things balance out, but in each individual case it is not really possible.

Mr. VALENTINE. Thank you.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Valentine.

Mr. Walker.

Mr. WALKER. Thank you, Madam Chairman. I apologize for being late. The Republican conference held me over. But I did have an "absolutely incisive" opening statement prepared by the minority staff that I would ask unanimous consent to be included in the——

Mrs. LLOYD. And we have been waiting patiently for your opening statement. [Laughter.]

Mr. WALKER. I am sure. And I did want to make certain it got into the record. I appreciate that.

Mrs. LLOYD. Without objection, it will be made a part of the record.

[The statement of Mr. Walker follows:]

OPENING REMARKS OF

REPRESENTATIVE ROBERT S. WALKER

COMPREHENSIVE PROGRAM MANAGEMENT PLAN

FOR MAGNETIC FUSION ENERGY

OCTOBER 20, 1983

THANK YOU, MADAME CHAIRMAN, AND LET ME ADD MY OWN WELCOME TO OUR WITNESSES BEFORE THE SUBCOMMITTEE .

OUR TOPIC TODAY WILL BE THE FUTURE ROLE OF GOVERNMENT SUPPORT OF THE FUSION ENERGY PROGRAM, AND THE PLACE FUSION ENERGY WILL HAVE IN MEETING OUR ENERGY NEEDS INTO THE NEXT CENTURY.

IN A WORLD IN WHICH OUR ENERGY RESOURCES ARE FINITE, AND RAPIDLY REACHING THE POINT OF CRITICALLITY, WE ARE EMBARKED ON A LONG AND DIFFICULT SEARCH FOR NEW AND UNLIMITED ENERGY SOURCES. FUSION

ENERGY SEEMS TO HAVE THE POTENTIAL TO MEET MAN'S FORESEEABLE ENERGY NEEDS FOR HUNDREDS OF YEARS.

COMMON SEAWATER IS THE FUEL FOR THE FUSION REACTION AND THE PROCESS HAS A NUMBER OF HIGHLY ATTRACTIVE FEATURES. PERHAPS THE MOST IMPORTANT IS THAT FUSION GENERATES MUCH LESS RADIOACTIVITY THAN FISSION, AND THERE IS NO LONG-TERM STORAGE PROBLEM FOR RADIOACTIVE WASTES. THERE ARE ALSO MAJOR ADVANTAGES IN THE FACT THAT A FUSION REACTOR IS FAR, FAR LESS CONTAMINATED BY RADIATION WHILE THE REACTOR IS BEING OPERATED. FOR EXAMPLE, A FUSION REACTOR MADE OF STAINLESS STEEL WOULD HAVE 300 TIMES LESS RADIOACTIVITY THAN A FISSION REACTOR OF THE SAME POWER OUTPUT. IF THAT SAME FUSION REACTOR WAS MADE OF VANADIUM, IT WOULD BE 10 TIMES BETTER YET. THEREFORE, IT IS CLEARLY POSSIBLE TO BUILD FUSION REACTORS WHICH WOULD HAVE 3,000 TIMES LESS RADIOACTIVITY THAN A FISSION REACTOR PRODUCING THE SAME AMOUNT OF POWER.

THIS QUESTION OF REDUCED RADIOACTIVITY IS OF MAJOR IMPORTANCE TO OUR FUTURE. WE ARE LEARNING AT THREE MILE ISLAND THAT THERE ARE A NUMBER OF TOUGH TECHNICAL ISSUES REVOLVING AROUND THE RADIOACTIVE CONTAMINATION PARTS OF THE STRUCTURE, PARTICULARLY THE DAMAGED CORE,

WHICH ARE CREATING MAJOR PROBLEMS.

THE RADIOLOGICAL DIFFERENCE BETWEEN FISSION AND FUSION IS EVEN MORE STRIKING IN THE PRODUCTION OF LONG-LIVED RADIOLOGICAL WASTES. THERE IS NOTHING IN THE FUSION REACTOR EVEN REMOTELY COMPARABLE TO THE FISSION FRAGMENTS OR THE PLUTONIUM IN FISSION REACTORS. PLUTONIUM IS EXTREMELY HAZARDOUS, AND ITS RADIOACTIVITY IS EXTREMELY LONG-LIVED, WITH A HALF-LIFE OF 24,100 YEARS. AFTER A 100 YEAR STORAGE PERIOD, THE RADIOACTIVE WASTES GENERATED BY A STAINLESS STEEL FUSION REACTOR WOULD BE ONE MILLION TIMES LESS HAZARDOUS THAN THE WASTES PRODUCED BY A FISSION REACTOR OF THE SAME GENERATION CAPACITY.

THERE ARE SEVERAL TECHNOLOGIES WHICH SEEM TO HAVE SUBSTANTIAL PROMISE FOR AN UNLIMITED ENERGY FUTURE, BUT NONE ARE AS SPECTACULAR AS FUSION ENERGY. THE ENERGY AVAILABLE FROM FUSING THE DEUTERIUM IN ONE CUBIC METER OF SEAWATER EQUALS THE ENERGY RELEASED BY BURNING 2,000 BARRELS OF CRUDE OIL. EVERY SINGLE CUBIC KILOMETER OF OCEAN WATER CONTAINS AS MUCH ENERGY AS THE ENTIRE KNOWN OIL RESERVES REMAINING ON EARTH, AND THERE ARE MORE THAN ONE BILLION CUBIC KILOMETERS OF SEA WATER IN THE OCEANS.

THE POTENTIAL FROM THIS TECHNOLOGY EXCEEDS MAN'S WILDEST DREAMS. UNLIMITED POWER FROM THE SEAS SEEMS TO BE THE ANSWER TO ALL OF OUR LONG-TERM ENERGY PROBLEMS. BUT MUCH REMAINS TO BE DONE BEFORE WE WILL KNOW IF IT WILL REALLY WORK. TODAY WE WILL COMMENCE OUR EXAMINATION OF THE COMPREHENSIVE PROGRAM MANAGEMENT PLAN FOR MAGNETIC FUSION ENERGY. THIS IS THE FIRST STEP IN DETERMINING EXACTLY WHERE THE PROGRAM IS TODAY, AND IN ESTABLISHING THE POLICIES WHICH WILL GUIDE OUR EFFORTS IN THE YEARS TO COME.

THANK YOU.

Mr. WALKER. Dr. Clarke, if the fusion energy program continues at the present level of effort, at about $470 million per year, how will you include a major new initiative such as the tokamak fusion core experiment?

Dr. CLARKE. There is no place within the level of effort that is represented by that number for a fusion core experiment. Our plan calls for taking that step some time in the 1990's, after we have been able to make a choice between some of the things that we're presently pursuing and, therefore, free up resources to take that step.

Mr. WALKER. So you don't see that as happening in conjunction with ongoing efforts; you are going to have to make some choices among priorities in the nineties is what you're saying?

Dr. CLARKE. That's correct.

Mr. WALKER. There is no mention in your prepared statement on college and university involvement in the fusion energy program. How many institutions are presently involved and how many professors are presently involved, and also how many students are presently active?

Dr. CLARKE. Well, I would have to provide the precise numbers for the record, if that's all right with you.

Mr. WALKER. That's fine.

[The material follows:]

Madame Chairman, in the fusion college and university propram:
There are 36 universities presently involved in the fusion energy program.
There are approximately 150 professors contributing to the program.
The number of graduate students is roughly 400. Between 70 and 80 Ph.D's are granted each year.

Dr. CLARKE. In general, I think we have over 25 different universities involved, and they range from very large programs such as the multimillion dollar, $20 million program at MIT, down to $100,000 contracts. It would take some time to count up all the professors and students, but there are quite a number.

Mr. WALKER. OK. And do you have some idea as to what the amount of funding is per year in that particular part of the program?

Dr. CLARKE. The precise dollar amount, again, I would have to provide you for the record.

Mr. WALKER. That's fine.

[The material follows:]

- For the university research groups, the FY1984 operating budget is $44.6 million, and the FY1984 capital equipment budget is $3.9 million.
- The percent support for universities in relation to the overall OFE budget is

	FY79	FY80	FY81	FY82	FY83	FY84
Operating	12%	12%	12%	13%	12%	11%
Equipment	13%	11%	8%	10%	11%	10%

Prior to 1979 there is no breakdown for university funding versus OFE total funding.

- The work in the U.S. fusion program is performed by the triad of national laboratories, industry, and universities. The strength of universities is different from that of either the national labs or industry. Universities serve as fertile ground to develop new ideas and to explore and understand basic science and technology. One benefit of university participation is that they train young professionals who can contribute in all three sectors of the working fusion community.

The universities also provide the community with a highly skilled group of scientists, with minimal vested interest, which is crucial to maintaining program quality and integrity.

Any trends that you see developing toward university involvement?

Dr. CLARKE. Not particularly. We have made an attempt over the past decade to maintain a constant percentage involvement of the university community in fusion. It's hard for me to separate out the different budget years. There are a few small percentage variations, but by and large, we have maintained a constant involvement of the university community over the last 10 years. I can provide you the detailed numbers.

Mr. WALKER. OK. I would appreciate that. I would hope we could keep the record open for those figures, Madam Chairman.

[The material follows:]

Future trends in magnetic fusion energy development imply fewer and larger experiments as the program evolves towards reactor conditions. Small and mid-sized experiments will continue to be needed to support the large devices, and some of these will be based at universities. It would also be beneficial for universities to engage in joint programs with the national labs and industry in the areas of design, construction, and operation.

In summary, we feel that the universities play a crucial role in the fusion community triad. The Office of Fusion Energy, considering the recommendations of the Magnetic Fusion Advisory Committee, will continue to support and encourage their participation.

Mrs. LLOYD. The record will be open for further questions from members as well.

Mr. WALKER. Thank you.

In the 1984 program summary for magnetic fusion energy, dated January 27, 1983, one vugraph stated, "We are now at a critical decade for industrial involvement." What industrial participation is contemplated in fiscal year 1984, and do you plan for increasing or decreasing industrial participation during the eighties?

Dr. CLARKE. Well, as indicated in my written statement, we have about 30 percent of the fusion program funds, present fusion program funds, spent in industry for various things, components for our facilities mostly.

What we mean by saying that this decade is a critical decade for industrial involvement is that we can look ahead and see that some time in the 1990's industry will be playing a major role as we get into the construction of this fusion core device, for example. This is clearly an industrial scale project. Today we have industries that are by and large handling the production of individual components, so there is a difference between what we have today and what we can foresee in the future. And when we refer to a critical decade, what we mean is that we need to make provision to have a smooth transition from today's involvement to the future.

As I indicated in my oral testimony, we have asked our Magnetic Fusion Advisory Committee to recognize that industrial involvement is an essential element in the future, to give us some specifics and to examine different modes of industrial involvement, so that we can have an efficient transition from where we are now to where we would like to be.

Mr. WALKER. Do I gather from your answer that the thing that you think can be handled most efficiently by industry are the fairly large-scale projects?

Dr. CLARKE. Yes, I think so—well, most efficiently, it depends. There are some industries, of course, that can do individual research tasks, and we do have some of those involved in fusion. But when I refer to these large projects, I'm looking at the unique characteristic of industry. Industry has the capacity for doing things

that our national laboratory, for example, just really can't handle or shouldn't handle. It's in that sense of unique capability.

Mr. WALKER. Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Walker.

One final question, John. How does the strategy of the Europeans and Japanese compare with our approaches as outlined in our management plan?

Dr. CLARKE. Let me think for a moment to be concise about this.

Mrs. LLOYD. Our approach, the steps that you have just outlined.

Dr. CLARKE. Well, a basic difference is that both Europe and Japan are presently putting their primary emphasis on tokamaks. The United States is carrying out two major lines of development in the tandem mirror and the tokamak. So this is a basic difference between our program and these other countries. In that sense, Europe and Japan are not precisely equal, but I think that's a general characterization.

So both of those countries have plans for completing their present series of experiments and then beginning a reactor scale step some time in the 1990's, So the next step device that is included, the fusion core experiment, in the comprehensive management plan in the United States, would be paralleled presumably by a major reactor step in both Europe and in Japan.

Mrs. LLOYD. So our core design step for the next decade will parallel——

Dr. CLARKE. Will parallel—I'm talking now about what their present plans are; I don't know what they'll actually do. But their present plans call for a next European TORUS in the 1990's and a fusion engineering reactor, I think, in Japan.

These devices are presently envisioned to be somewhat like INTOR, which would be more elaborate than our present concept of the TFCX. It would involve more engineering technology. So there is a difference in that sense between the anticipated next steps of these programs.

Mrs. LLOYD. If you just had a choice, their strategy and our strategy, do you still prefer our strategy and our plan?

Dr. CLARKE. I have to answer that, under the constraints and the circumstances——

Mrs. LLOYD. The budgetary constraints, the financial——

Dr. CLARKE [continuing]. We find ourselves in. I think our plan is the only thing we have been able to think of that will maintain progress in the program, maintain the core of people that are necessary to carry out fusion, and I'm afraid that's what we end up with.

Mrs. LLOYD. Well, thank you very much. Thank you for the good work, hard work you have put into the fusion program. We appreciate you and thank you for your testimony.

Our next witness is Dr. Ronald Davidson, who is Chairman of the Magnetic Fusion Advisory Board for the Department of Energy. We welcome you, Dr. Davidson, and please proceed with your testimony.

STATEMENT OF RONALD C. DAVIDSON, CHAIRMAN, MAGNETIC FUSION ADVISORY COMMITTEE, U.S. DEPARTMENT OF ENERGY

Dr. DAVIDSON. Madam Chairman and committee members, I thank you for this opportunity to describe the fusion program priorities and strategy recommended by the Magnetic Fusion Advisory Committee.

MAGNETIC FUSION ADVISORY COMMITTEE

CHARTERED AREAS OF ADVICE AND GUIDANCE TO DOE

- Periodic reviews of elements of the program and recommendation of any changes considered desirable on the basis of scientific and technological advances or other factors
- Advice on long-range plans, priorities, and strategies to demonstrate the scientific and engineering feasibility of fusion
- Advice on recommended appropriate levels of funding to develop those strategies and to help maintain appropriate balance between competing elements of the program
- Advice and guidance on any issues relating to the program as requested by the Secretary or the Director of Energy Research

FIGURE 1

If I could have the first vugraph (fig. 1). The Magnetic Fusion Advisory Committee has several chartered areas of advice and guidance to the DOE Director of the Office of Energy Research. These are outlined in the first vugraph. They include periodic reviews of elements of the program, advice on long-range plans, priorities and strategies, advice on recommended appropriate levels of funding to develop those strategies, and to help maintain appropriate program balance.

MAGNETIC FUSION ADVISORY COMMITTEE

Ronald C. Davidson, Chairman Massachusetts Institute of Technology	John R. Gilleland G.A. Technologies
Harold K. Forsen, Vice-Chairman Bechtel	Robert A. Gross Columbia University
Lee A. Berry Oak Ridge National Laboratory	John W. Landis Stone and Webster
Renso Caporali Grumman Aerospace	Rulon K. Linford Los Alamos National Laboratory
Robett W. Conn University of California at Los Angeles	Peter Murray Westinghouse
John E. Cummings Electric Power Research Institute	Marshall N. Rosenbluth University of Texas
T. Kenneth Fowler Lawrence Livermore National Laboratory	Alan Schriesheim Argonne National Laboratory
Harold P. Furth Princeton University	Bobby J. Tharpe General Electric

FIGURE 2

The second vugraph (fig. 2) gives a summary of the present membership of the Magnetic Fusion Advisory Committee. You can see that there is very broad representation from industry, the laboratories, and the universities. Since its inception in June 1982, we have met in public session on seven occasions. We receive charges from the Director of the Office of Energy Research. We establish subpanels which thoroughly research the various charge areas and these subpanels report back to the Magnetic Fusion Advisory Committee where we, in turn, formulate recommendations to the Department of Energy.

	AREA OF MFAC EVALUATION	PANEL CHAIRPERSON	DOE CHARGE RECEIVED	MFAC RECOMMENDATIONS TO DOE
#1	Tandem Mirrors & Tokamaks	Dr. John Gilleland G.A. Technologies	June, 1982	November, 1982
#2	Alternate Confinement Concepts	Prof. Robert Conn UCLA	June, 1982	November, 1982
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FIGURE 3

The next vugraph (fig. 3) shows the various area of evaluation that have been carried out by MFAC during the past 15 months. Charge area number seven was mentioned earlier by Dr. Clarke. It was a charge that we received in September 1983, and the subpanel looking into that charge is Dr. Harold Forsen of Bechtel. The first six charge areas have been completed and, briefly, they are the following:

We have carried out a detailed review of the tandem mirror and tokamak confinement programs, of the alternate concepts, of upgrade options for TFTR, of upgrade options for MFTF-B. We have also looked at the long-term role of universities in the fusion program, and item No. 6, which is the one that I would like to focus on today, we were asked to integrate and prioritize all of the findings and recommendations in the first charge areas. So in a few vugraphs I would like to essentially summarize 15 months of work by this committee and the subpanels——

Mrs. LLOYD. Very good, and obviously you have been very busy.

Dr. DAVIDSON. If I could have the next vugraph (fig.4).

BACKGROUND OF PROGRAM PLANNING

1. To realize the objective of a working demonstration reactor in a cost-effective manner, there must be strong and timely emphasis on the development of practical fusion hardware. (ERAB Fusion Review Panel, 1980; Magnetic Fusion Energy Engineering Act of 1980.)

2. Essential need to support a "broad-based but tightly integrated research program" in magnetic confinement. (MFAC, October, 1982.)

3. Next major program step should be a "near-term demonstration of ignition and long-pulse equilibrium burn". (MFAC, January, 1983.)

4. Overall program planning must be carried out within appropriate budgetary constraints. (CPMP, 1983.)

5. Take maximum advantage of existing facilities and of opportunities for the achievement of integrated science/technology and engineering objectives (MFAC Panels 3 and 4).

FIGURE 4

As background information on program planning, we have had several sources of input. The 1980 ERAB fusion review panel and the Magnetic Fusion Engineering Act of 1980 recommended a strong and timely emphasis on the engineering development of practical fusion hardware. Item No 2, in October of 1982 the Magnetic Fusion Advisory Committee recommended that it was very important to maintain a broad based, but tightly integrated, research program in magnetic fusion. Item No. 3, also one of the MFAC findings, in January of 1983 we recommended that the next major program step should be a near-term demonstration of ignition and long-term equilibrium burn. Item No. 4, we recognize that overall program planning must be carried out within appropriate budgetary constraints, which were outlined in the CPMP of 1983. And the fifth area, the Magnetic Fusion Advisory Committee also recommended that we should take maximum advantage of existing facilities and of opportunities for the achievement of integrated science and technology and engineering objectives.

The final two vugraphs give a summary of the program and strategy that we recommend. Next vugraph, please (fig. 5).

SUMMARY OF PROGRAM AND STRATEGY

RECOMMENDED BY MFAC

1. Initiation in FY86 of the Tokamak Fusion Core Experiment (TFCX), a moderate-cost tokamak reactor device (less than $1 B PACE) designed to achieve ignition and long-pulse equilibrium burn. Careful trade-off studies are needed before making key design choices in interrelated technology areas. Cost reductions relative to earlier plans can be realized by exploiting new plasma technology, by locating the TFCX at the TFTR site, and by assigning responsibility for complementary reactor engineering tasks to other sectors of the fusion program.

2. Potential utilization of the MFTF Upgrade to provide a cost-effective means for quasi-steady-state testing of blanket and power-system components, complementary to TFCX. This will depend on future assessments of the data base for tandem mirrors.

3. Vigorous pursuit of the broad U.S. base program in magnetic confinement, including new machine starts, where appropriate, at approximately the present total level of support.

4. Utilization of Development and Technology programs in plasma and magnet technology in support of specific hardware requirements of the TFCX and of other major fusion facilities, so as to minimize overall program cost.

FIGURE 5

We recommend initiation in fiscal 1986 of the tokamak fusion core experiment, which we view as a moderate-cost tokamak reactor device, costing somewhat less than $1 billion in PACE funds, and the main objective of this experiment would be to achieve ignition and long-pulse equilibrium burn. Cost reductions relative to earlier plans can be realized by exploiting new plasma technology, by locating the TFCX at the TFTR site, and by assigning responsibility for complementary reactor engineering tasks to other sectors of the fusion program.

For example, in item No. 2, depending on future favorable assessments of the data base for tandem mirrors, the MFTF-B facility could be upgraded to provide a cost-effective means for testing of blanket and power system components which would play a complementary role to the TFCX.

The third element of our recommended program and strategy relates to maintaining a vigorous and broad-based program in mag-

netic confinement, including new machine starts, where appropriate, at approximately the present level of total support.

And item No. 4 is a recommendation that some of the development and technology programs could be utilized in support of specific hardware requirements of the TFCX and of other major fusion facilities, so as to minimize overall program cost.

SUMMARY OF PROGRAM AND STRATEGY

RECOMMENDED BY MFAC

- The immediate need is to take a strong initiative in reactor-oriented research byond the TFTR, JET, JT-60, T-15 generation of tokamak facilities. The TFCX project has broad and enthusiastic support in the U.S. fusion community. There is a consensus that the data base now exists for this step, with final confirmation to be expected from TFTR in the next two years. It is clear that a delay in decision making would not add to the critical data base and would only lead to loss of momentum in the U.S. program and loss of the techincal capability for leadership in world fusion research.

- Carrying out the above program will require a budget that ramps up over a period of several years to a new budget level 25-40 percent above the present level in constant dollars. As a first step, in FY85 new funding will be needed to support a major joint effort by industry and the national fusion community to prepare an optimized conceptual design for the TFCX, including detailed determinations of scope and cost. These new funds for the TFCX project are needed in addition to sustained support of the base program: that is, timely completion of MFTF-B and effective pursuit of the currently active experimental programs in tokamak, mirrors, and alternate concepts, as well as the supporting work in basic plasma research and fusion technology. With current inflation taken into account, the minimum recommended FY85 budget should be $535 M, which is an increase of about 15 percent above the FY84 level.

FIGURE 6

On the final vugraph there are two major points (fig. 6). The TFCX project has broad and enthusiastic support in the U.S. fusion community. There is a consensus that the data base now exists for this step, with final confirmation to be expected from TFTR in the next few years. It is also our strong view that embarking on such an ignition experiment is very important with regard to the United States maintaining world leadership in fusion during the 1990's.

The budget implications for what we are recommending is that to carry out the program that we recommend will require a budget that ramps up over a period of several years to a new budget level somewhere in the range 25 to 40 percent above the present level in constant dollars. So the program that we're recommending is intermediate between the aggressive Fusion Engineering Act of 1980 and the flat projections of this administration.

[The material referred to follows:]

ASSUMED BUDGET LEVELS

IN FORMULATING MFAC RECOMMENDATIONS

The program plans and priorities recommended by MFAC have been prepared for two budget cases:

1. A "base" case for the fusion program that corresponds to a cost-of-living increase in FY85 (to approximately $500 M) over the level in FY84 ($471 M), with approximately constant funding in the "out" years (FY86 and beyond).

2. An "enhanced" budget case, which ramps up over a period of several years to a new budget level 25-40 percent above the present level in constant dollars.

Mrs. LLOYD. It is my understanding the base program is about—that is being projected—is about $20 million lower than you recommend.

Dr. DAVIDSON. You mean in fiscal 1985?

Mrs. LLOYD. Yes.

Dr. DAVIDSON. I will get to that recommendation in just a second.

Mrs. LLOYD. Excuse me.

Dr. DAVIDSON. In fiscal 1985 new funding will be needed to support a major joint effort by industry and the national fusion community to prepare an optimized conceptual design for the TFCX. These new funds for the TFCX project are needed in addition to sustained support for the base program; that is to say, timely completion of the MFTF-B and effective pursuit of the currently active experimental programs in tokamak, mirrors, and alternate concepts, as well as supporting work in basic plasma research and fusion technology. With current inflation taken into account, the minimum recommended fiscal 1985 budget should be $535 million, which is an increase of about 15 percent above the fiscal 1984 level.

As I said earlier, in the plan that we're proposing there would be a ramp up over a period of several years to a level which is in the range 25 to 40 percent higher than the present program level, and

whether it's 25 or whether it's 40 percent depends on the design option chosen for the TFCX.

That concludes what I would like to say. I would just reiterate the point that the plan that we are proposing is intermediate between the aggressive plan in the Engineering Act of 1980 and the flat situation as envisioned in the CPMP over the next several year period. The program is ready to embark on the TFCX and we believe that that commitment should be made now. The design funds in fiscal 1985, project approval in fiscal 1986, the program is ready to do that and, unlike the CPMP, that should be now, rather than wait for the early nineties.

[The prepared statement of Dr. Davidson follows:]

MAGNETIC FUSION ADVISORY COMMITTEE

RECOMMENDED FUSION PROGRAM PRIORITIES AND STRATEGY

Presented to

U.S. HOUSE OF REPRESENTATIVES
COMMITTEE ON SCIENCE AND TECHNOLOGY
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

Presented by

Ronald C. Davidson

October 20, 1983

MAGNETIC FUSION ADVISORY COMMITTEE

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- Advice on recommended appropriate levels of funding to develop those strategies and to help maintain appropriate balance between competing elements of the program

- Advice and guidance on any issues relating to the program as requested by the Secretary or the Director of Energy Research

MAGNETIC FUSION ADVISORY COMMITTEE

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Massachusetts Institute of Technology

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Renso Caporali
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Robert W. Conn
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John W. Landis
Stone and Webster

Rulon K. Linford
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Peter Murray
Westinghouse

Marshall N. Rosenbluth
University of Texas

Alan Schriesheim
Argonne National Laboratory

Bobby J. Tharpe
General Electric

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ASSUMED BUDGET LEVELS IN FORMULATING MFAC RECOMMENDATIONS

The program plans and priorities recommended by MFAC have been prepared for two budget cases:

1. A "base" case for the fusion program that corresponds to a cost-of-living increase in FY85 (to approximately $500 M) over the level in FY84 ($471 M), with approximately constant funding in the "out" years (FY86 and beyond).

2. An "enhanced" budget case, which ramps up over a period of several years to a new budget level 25-40 percent above the present level in constant dollars.

BACKGROUND OF PROGRAM PLANNING

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4. Overall program planning must be carried out within appropriate budgetary constraints. (CPMP, 1983.)

5. Take maximum advantage of existing facilities and of opportunities for the achievement of integrated science/technology and engineering objectives (MFAC Panels 3 and 4).

SUMMARY OF PROGRAM AND STRATEGY RECOMMENDED BY MFAC

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2. Potential utilization of the MFTF Upgrade to provide a cost-effective means for quasi-steady-state testing of blanket and power-system components, complementary to TFCX. This will depend on future assessments of the data base for tandem mirrors.

3. Vigorous pursuit of the broad U.S. base program in magnetic confinement, including new machine starts, where appropriate, at approximately the present total level of support.

4. Utilization of Development and Technology programs in plasma and magnet technology in support of specific hardware requirements of the TFCX and of other major fusion facilities, so as to minimize overall program cost.

SUMMARY OF PROGRAM AND STRATEGY RECOMMENDED BY MFAC

- The immediate need is to take a strong initiative in reactor-oriented research beyond the TFTR, JET, JT-60, T-15 generation of tokamak facilities. The TFCX project has broad and enthusiastic support in the U.S. fusion community. There is a consensus that the data base now exists for this step, with final confirmation to be expected from TFTR in the next two years. It is clear that a delay in decision making would not add to the critical data base and would only lead to loss of momentum in the U.S. program and loss of the technical capability for leadership in world fusion research.

- Carrying out the above program will require a budget that ramps up over a period of several years to a new budget level 25-40 percent above the present level in constant dollars. As a first step, in FY85 new funding will be needed to support a major joint effort by industry and the national fusion community to prepare an optimized conceptual design for the TFCX, including detailed determinations of scope and cost. These new funds for the TFCX project are needed in addition to sustained support of the base program: that is, timely completion of MFTF-B and effective pursuit of the currently active experimental programs in tokamak, mirrors, and alternate concepts, as well as the supporting work in basic plasma research and fusion technology. With current inflation taken into account, the minimum recommended FY85 budget should be $535 M, which is an increase of about 15 percent above the FY84 level.

September 28, 1983

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee met in Cambridge, Massachusetts on September 27 and 28 and reviewed the findings and recommendations of the MFAC Panel evaluating fusion program balance and priorities (Panel #6).

The Magnetic Fusion Advisory Committee endorses the principal findings and recommendations of the Panel (see attached Executive Summary and Panel Report) and urges the Department of Energy to take immediate steps to implement the enhanced program, focussed on the Tokamak Fusion Core Experiment (TFCX), as recommended in the Panel Report.

The TFCX has the unanimous and enthusiastic endorsement of MFAC as the necessary next major step. Taking timely advantage of the growing technical data base, the TFCX project will restore the forward thrust of the U.S magnetic fusion program and advance it towards the goal of a working reactor.

The Committee also concludes that initiation of the TFCX will require significant incremental funding. After fifteen months of careful examination of the content and balance of the fusion program, we find that the program is making effective use of its present resources, and that the overall balance, priorities and level of effort in the base program are approximately correct. Consequently, the enhanced program that we recommend will require a budget that ramps up over a period of several years to a new budget level, 25-40 percent above the present level in constant dollars.

Finally, we note that some elements of the applied plasma physics and development and technology base research programs have not been included in the Panel #6 review process. The essential role that many of these programs play as a source of innovative ideas and scientific and technological advances in the fusion program is documented insofar as University programs are concerned in the MFAC Panel #5 Report forwarded to you in August, 1983.

I look forward to discussing our recommendations with you at your earliest convenience.

Sincerely,

Ron Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion
Advisory Committee

RCD:gnc

September 30, 1983

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

The recommendations of the Magnetic Fusion Advisory Committee (MFAC) concerning the Tokamak Fusion Core Experiment (TFCX) were reported to you in my letter of Septeber 28, 1983. I am sending you this supplementary note to highlight some relevant details of the program funding plan proposed in the MFAC Panel #6 report.

Present TFCX scoping studies are addressing a set of design options that range from about $500 M to $1000 M in PACE cost. The associated operating costs during TFCX construction are estimated at $350 M to $500 M; the total project costs are in the range of $850 M to $1500 M. These estimates are based on the assumption that a direct saving of $200 M - $300 M is effected by reutilization of TFTR (or equivalent) facilities; they do not include an allowance for institutional costs for start-up of a major new research site.

As outlined in the Panel #6 report (particularly, in Secs. 1.4 and 2.2, Appendix A, and related tables), a substantial part of the required TFCX construction funding could be obtained from within the presently projected MFE budget. During the proposed TFCX construction period ('86-'92), a total of up to $400 M could be made available by sacrificing TFTR experimental operation and terminating the TFTR project as early as FY'90. In addition, it is estimated that a total of more than $100 M of current funding in the Development and Technology areas could be redirected in support of TFCX R & D costs.

In the present phase of world fusion research, tokamak projects in Europe and Japan are approaching the $1000 M level of construction costs. Taking advantage of new concepts and techniques in the area of plasma engineering, it should be possible to achieve a truly significant advance over the present state of the art in a new facility of about $800 M PACE cost - corresponding to about $1500 M of total investment value. While a large part of this cost would be met by reusing existing facilities and redirecting present budgets, we estimate that a total MFE budget increment in the range of at least $700 M, spread over the years FY'86 - '92, would be required for the timely and cost-effective implementation of the TFCX project.

Sincerely,

Ron Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committe

RCD:gnc

August 3, 1983

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C., 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee met in San Francisco on August 2 and 3 and received input from Panel No. 6, which is formulating detailed recommendations to MFAC on fusion program priorities and overall strategy. It is anticipated that a final report will be submitted to MFAC on this subject by the end of September. Meanwhile we would like to keep you informed of the general nature of the recommendations that are under development.

The Panel 6 deliberations are based on a set of four major premises:

1. Need for vigorous progress in the development of fusion plasma technology and nuclear technology. (ERAB Fusion Review Panel, 1980; Fusion Engineering Act of 1980).

2. Maintenance of a "broad-based but tightly integrated research program" in magnetic confinement to serve as the primary source of fruitful new ideas and program directions. (MFAC, October, 1982).

3. Emphasis on a "near-term demonstration of ignition and long-pulse equilibrium burn" as the technically necessary next major program step. (MFAC, January, 1983).

4. Constraint of fusion program planning within realistic overall budgetary guidelines. (CPMP, 1983).

On the basis of these four premises, Panel 6 is developing a new strategy with the following principal features:

1. Initiation in FY'86 of the TFCD, a moderate-cost tokamak reactor device (less than $1B PACE) designed to achieve ignition and long-pulse equilibrium burn. Cost reductions relative to earlier plans can be realized by exploiting new plasma technology, by locating the TFCD at the TFTR site, and by assigning responsibility for complementary reactor engineering tasks to other sectors of the fusion program.

2. Potential utilization of the MFTF Upgrade to provide a cost-effective means for quasi-steady-state testing of blanket and power-system components, complementary to TFCD. This will depend on future assessments of the data base for tandem mirrors.

3. Vigorous pursuit of the broad U.S. base program in magnetic confinement, including new machine starts where appropriate, at approximately the present total level of support. The continued flow of innovative ideas and new results is essential to the optimization of the ultimate reactor product.

4. Utilization of D&T programs in plasma and magnet technology in support of specific hardware requirements of the TFCD and of other major fusion facilities, so as to minimize overall program cost.

Timely execution of this near-term strategy is consistent with the achievement of an optimized experimental fusion reactor to provide the basis for an assessment of the potential of fusion power around the turn of the century. The demonstration of net power production in a complete working system is an essential prerequisite for significant industrial investment in fusion power.

The immediate need is to take a strong initiative in reactor-oriented research beyond the TFTR, JET, JT-60, T-15 generation of tokamak facilities. The TFCD project has broad and enthusiastic support in the U.S. fusion community. There is a consensus that the data base now exists for this step, with final confirmation to be expected from TFTR in the next two years. It is clear that a delay in decision making would not add to the critical data base and would only lead to loss of momentum in the U.S program and loss of the technical capability for leadership in world fusion research. This leadership has been based on the capital investments of the 70's, and this capital is now nearly exhausted.

Carrying out the above program will require a budget increase of 25-40 percent, after inflation, over a period of several years. As a first step, in FY'85 new funding will be needed to support a major joint effort by industry and the national fusion community to prepare an optimized conceptual design for the TFCD, including detailed determinations of scope and cost. These new funds for the TFCD project are needed in addition to sustained support of the base program: that is, timely completion of MFTF-B and effective pursuit of the currently active experimental programs in tokamak, mirrors, and alternate concepts, as well as the supporting work in basic plasma research and fusion

technology. With current inflation taken into account, the minimum FY'85 budget should have an increase of about 15 percent above the FY'84 level: Base Program $510 M; TFCD Design Activities $25 M; Total $535 M.

We hope that you will find this interim report timely and helpful.

Sincerely,

Ronald C. Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee

RCD:pt

EXECUTIVE SUMMARY

MAGNETIC FUSION ADVISORY COMMITTEE

RECOMMENDED FUSION PROGRAM PRIORITIES AND STRATEGY

September, 1983

I. SUMMARY OF RECOMMENDED PROGRAM

Background

In seeking to formulate a magnetic fusion program strategy for the next decade, the Magnetic Fusion Advisory Committee has benefited from strategies developed in previous planning studies and deliberations as follows:

1. To realize the objective of a working demonstration reactor in a cost-effective manner, there must be strong and timely emphasis on the development of practical fusion hardware: advanced plasma-engineering hardware to produce and contain real reactor plasmas; advanced nuclear hardware to control and utilize the output of fusion reactor cores. (ERAB Fusion Review Panel, 1980; Magnetic Fusion Energy Engineering Act of 1980.)

2. To insure optimization of the ultimate fusion reactor product by a continued flow of fruitful new ideas and program directions, there is an essential need to support a "broad-based but tightly integrated research program" in magnetic confinement. (MFAC, October, 1982.)

3. To take best advantage of current frontline advances in fusion science and technology, the next major program step should be a "near-term demonstration of ignition and long-pulse equilibrium burn." (MFAC, January, 1983.)

4. To insure realism and continuity of the U.S. magnetic fusion effort, overall program planning must be carried out within appropriate budgetary constraints. (CPMP, 1983.)

5. Program planning should seek to take maximum advantage of existing facilities and of opportunities for the achievement of integrated science/technology and engineering objectives (MFAC Panels #3, #4, and #6).

Summary of Recommended Program

With this background in mind, the Magnetic Fusion Advisory Committee recommends a new program strategy with the following principal features:

1. Initiation in FY86 of the Tokamak Fusion Core Experiment (TFCX), a moderate-cost tokamak reactor device (less than $1 B PACE) designed to achieve ignition and long-pulse equilibrium burn. Careful trade-off studies are needed before making key design choices in interrelated technology areas. Cost reductions relative to earlier plans can be realized by exploiting new plasma technology, by locating the TFCX at the TFTR site, and by assigning responsibility for complementary reactor engineering tasks to other sectors of the fusion program.

2. Potential utilization of the MFTF Upgrade to provide a cost-effective means for quasi-steady-state testing of blanket and power-system components, complementary to TFCX. This will depend on future assessments of the data base for tandem mirrors.

3. Vigorous pursuit of the broad U.S. base program in magnetic confinement, including new machine starts, where appropriate, at approximately the present total level of support.

4. Utilization of Development and Technology programs in plasma and magnet technology in support of specific hardware requirements of the TFCX and of other major fusion facilities, so as to minimize overall program cost.

Further details of these recommendations, and supplementary recommendations concerning other specific program areas, are contained in Secs. 2 and 3 and Appendices A-D of the Panel #6 Report.

The immediate need is to take a strong initiative in reactor-oriented research beyond the TFTR, JET, JT-60, T-15 generation of tokamak facilities. The TFCX project has broad and enthusiastic support in the U.S. fusion community. There is a consensus that the data base now exists for this step, with final confirmation to be expected from TFTR in the next two years. It is clear that a delay in decision making would not add to the critical data base and would only lead to loss of momentum in the U.S. program and loss of the technical capability for leadership in world fusion research.

Carrying out the above program will require a budget that ramps up over a period of several years to a new budget level 25-40 percent above the present level in constant dollars. As a first step, in FY85 new funding will be needed to support a major joint effort by industry and the national fusion community to prepare an optimized conceptual design for the TFCX, including detailed determinations of scope and cost. These new funds for the TFCX project are needed in addition to sustained support of the base program: that is, timely completion of MFTF-B and effective pursuit of the currently active experimental programs in tokamak, mirrors, and alternate concepts, as well as the supporting work in basic plasma research and fusion technology. With current inflation taken into account, the minimum recommended FY85 budget should be $535 M, which is an increase of about 15 percent above the FY84 level.

II. PRINCIPAL FINDINGS, RECOMMENDATIONS AND PRIORITIES

A. Tokamaks and Tandem Mirrors

This section gives a summary of selected principal findings, recommendations and priorities for tokamaks and tandem mirrors. These are developed in more detail in Sec. 2 and in Appendices A and B of the Panel #6 Report.

1. The tandem mirror and tokamak concepts can be embodied in viable commercial reactors of roughly similar projected cost and plant capacity (1000 MW electric), but with their own distinct topologies, technical features and operating approaches. In both reactor designs, there are scientific and technological assumptions that remain to be validated by experiment. Both designs also have the potential for significant further improvement through technical innovations that are in the exploratory phase.

2. The U.S. magnetic fusion program derives important benefits from pursuing the toroidal and mirror programs in parallel: the fundamental differences between the two types of magnetic confinement concept tend to provide insurance against the emergence of common technical obstacles; collaboration in the development of physics insights and competition in the pursuit of reactor attractiveness have served to stimulate progress in both programs.

3. Tokamaks are at a more advanced stage of development than tandem mirrors. In the case of the tokamak, the existing data base implies the feasibility of net power production. The recently completed TFTR facility is expected to achieve the world's first significant production of fusion power in 1986.

4. The near-term demonstration of ignition and long pulse equilibrium burn constitutes the technically necessary next major step for tokamaks beyond the TFTR break-even demonstration. The TFCX will permit a thorough characterization of the plasma core of a fusion reactor and of many reactor prototypical engineering features. Moreover, it will provide renewed impetus to fusion research and will facilitate a more aggressive community stance with respect to ETR objectives. Reactor prototypical features and systems should be employed to the maximum possible extent in the TFCX design, as long as this does not greatly increase project cost or technical risk. Careful trade-off studies are needed before making key design choices in interrelated areas such as magnet technology, nuclear technology, and maintenance approach.

5. Tandem mirror research is at an earlier stage of development than tokamaks. The current experimental program is addressing the feasibility of net power production. The possibility of achieving the required quality of reactor-plasma confinement will be put to critical tests, first on TMX-U (now in progress) and then definitively on the MFTF-B facility, which is to begin operation in 1986.

6. MFTF-B will remain a unique facility in the fusion program for the forseeable future and full advantage should be taken of this resource to investigate mirror physics and to advance fusion technology development. Of the MFTF-B upgrade options considered, the α + T option has several attractive features, including the potential capability of testing nuclear technology issues at reactor-level conditions. The tandem mirror community should continue a vigorous study program investigating possible upgrades of MFTF-B including higher-Q options.

7. The necessary next major step for the U.S. program is the TFCX. Priority for this project is second only to maintenance of a vigorous base program. In the enhanced program, highest priority for incremental funding should be given to the TFCX and the required confinement and D&T programs in support of that facility. As discussed in Sec. 1 and in Sec. 2.2 of the Panel #6 Report, significant incremental funds are clearly required for implementation of the TFCX.

8. The maintenance of a vigorous base program is essential to the technical progress in tokamak and tandem mirror research. Moderate-sized research facilities are the primary sources of the scientific and technological innovations required to develop fusion to its fullest potential. These programs also provide the detailed data base and operating experience underlying the designs and definitive tests on large-scale facilities. The recommended matrix of base program confinement experiments outlined in Secs. 2.2 and 2.3 and in Appendices A and B of the Panel #6 Report represent a tightly integrated base program that should receive high funding priority, together with the supporting research in applied plasma physics and development and technology.

9. The limitations imposed by a constant budget in the tokamak base program give rise to a number of critical shortfalls in key areas where major technical opportunities exist. Incremental funding of the tokamak base program would serve to insure the timely completion of the TFTR $Q \sim 1$ demonstration and the vigorous pursuit of the two most important new initiatives in tokamak experimental research (cf., Sec 2.2 and Appendix A of the Panel #6 Report): the Alcator DCT facility and the National ICRF Project.

10. In view of the premier role of MFTF-B as the only major scaling experiment in the tandem mirror program worldwide, incremental funding in the tandem mirror program should first be used to minimize delays in the MFTF-B schedule. Depending on favorable assessments of the tandem mirror data base, high priority for incremental funding in the enhanced program should also be given to the MFTF Upgrade. Study of upgrade options should continue; in formulating these options, it is particularly important to quantify the programmatic value of large-scale nuclear testing in MFTF Upgrade.

B. Alternate Concepts

This section gives a summary of the principal findings, recommendations and priorities for the alternate concepts. The reactor prospects, development status and recommended research programs are described in Sec. 3 and in Appendix D of the Panel #6 Report.

1. Research on the alternate concepts has resulted in significant advances in the basic physics understanding of plasma confinement properties, as well as the development of concepts that show significant promise of an improved reactor product. Moreover, the technology development and innovative reactor features associated with the alternate concepts have had a favorable impact on the development of the mainline approaches. The physics and technology payoff of such independent programs is large.

2. Assuming continued strong technical progress, further development of the alternate concepts is recommended in both the "enhanced" and "base" cases. The pace of development should be based on technical merit. Even at "reduced" budget levels, it is important that at least the present fraction of the confinement effort be devoted to experimentation on alternate concepts.

3. Because of the inflexibility in the alternate concepts base budget, and the importance of the alternates to the total program, it is necessary to give special consideration to the alternates when allocating incremental funding to the total base program. The Reversed Field Pinch program is expecced to be ready for such consideration in the near term, with other concepts to follow. Therefore, at present, the highest priority for incremental funding in the alternate concepts area is to proceed with IT-H, assuming favorable technical reviews.

4. There has been substantial technical progress in the reversed field pinch data base. Moreover, the reversed field pinch has the potential to produce a qualitatively different reactor product, namely a high-power-density,

high-beta, compact reactor with prospects for ignition solely with ohmic heating. For these reasons, high priority should be given to further development of the reversed field pinch concept. Favorable confinement scaling with increased current is important and, if technically feasible, should be evaluated in ZT-40 and other experiments as soon as possible. In parallel, there should be an assessment of the engineering issues related to the compact high-power-density aspects of reversed field pinch experiments and reactor designs. The reversed field pinch "reference" program* has technical merit for the "base" case, whereas the reversed field pinch "enhanced" program* appears appropriate to the "enhanced" fusion program level. The next step in the experimental reversed field pinch program should be pursued as soon as possible, assuming favorable technical reviews.

5. Based on the present understanding, the conventional stellarator leads to a reactor product qualitatively similar to the mainline approaches, with exceptionally low requirements for recirculating power but with difficult engineering features related to coil design. Conventional stellarator approaches are being actively pursued in Japan, the Federal Republic of Germany and the U.S.S.R. Because of the close relationship between stellarator and tokamak physics, including a similarity of techniques for achieving high-beta stabilization, a stellarator experiment (the ATF-1 torsatron at ORNL) has been approved as part of the tokamak program as an upgrade of the ISX-B tokamak facility. In the ATF-1, priority will be given to the advancement of the basic understanding of toroidal confinement, as well as the development of stellarator and stellarator-tokamak hybrid options for the longer term. Novel theoretical stellarator concepts for optimizing beta values and coil geometry should also continue to be explored.

6. The standard Elmo Bumpy Torus is a steady-state, modular concept that offers a product qualitatively similar to the mainline approaches, namely, a low-to-moderate power density, large-scale power reactor. In a recent review of the EBT-S data base,** it was concluded that the energy and particle confinement times in EBT-S were approximately 1 ms and that a verification of neoclassical scaling in EBT-S has not been made. We endorse the principal conclusions** of the EBT Senior Review Committee that: (a) ORNL and other members of the EBT community give top priority in the next 12 to 18 months

* Appendix C of the Panel #2 Report.

** EBT Data Review Panel (February, 1983), and EBT Senior Review Committee (February, 1983).

to improving the performance and the understanding of performance on EBT-S; (b) it (should) be understood from the outset that future steps in the EBT program will be decided on the basis of the outcome of this effort; and (c) the innovative thinking already in progress on reactor concept improvement should receive continued emphasis in parallel with the intensified experimental program on EBT-S. If the outcome of these experimental studies and the concept improvements is sufficiently promising, it is anticipated that the next appropriate step in the EBT program would be to test the new concept in an EBT-S sized experiment.

7. Compact toroids offer a qualitatively different reactor product, including simpler geometry and compact, high-power-density systems. The data base for the spheromak and field reversed configuration has been increased substantially but further progress is needed before technical selection of plasma and reactor configurations can be made. For these reasons the existing facilities with upgrades as appropriate should be fully utilized to address the outstanding issues. Continued emphasis should be placed on confinement scaling while maintaining gross stability, and on reactor relevant formation and sustainment methods.

III. RECOMMENDED PROGRAM OBJECTIVES AND POLICIES

The following is a summary of the overall fusion program objectives and policies recommended by the Magnetic Fusion Advisory Committee.

Objectives: Establishing the data base for the economic assessment of fusion power should be the fundamental program objective. The demonstration of a working fusion reactor is one of two essential prerequisites for attaining this objective. The other prerequisite is a sufficiently thorough understanding of fusion science and technology so that the reactor demonstration can be placed in the broader context of the potential long-term development of fusion power.

Program Breadth: Maintenance of a broad approach to magnetic fusion is absolutely essential (see next item). Outstanding experimental success should be followed up according to their own requirements and opportunities -- not constrained in favor of equalization.

Ranking of Alternates: It is particularly difficult to quantify the details of commercial reactor performance for emerging concepts. The Magnetic Fusion Advisory Committee recommends that the principal emphasis be placed on developing breadth and depth of technical insight for those concepts that could lead either to an improved reactor product in their own right or to improvements in the mainline approaches.

Balance Between Science and Technology: The apparent conflict between the development of fusion science and technology can be reconciled, to a large extent, by means of integrated planning. In the near term, a productive way to advance relevant fusion technology is to emphasize development of components needed for major new fusion experiments whenever possible. Independent development and technology activities in complementary areas, such as materials, however, will also be required. In the longer term, the success of reactor plasma experiments will provide advantageous new facilities for reactor engineering studies and the development of an engineering data base.

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Mrs. LLOYD. Thank you. Thank you for your work and for your insight and your testimony as well. I think that your plan certainly is logical and certainly does make sense.

I would ask you basically what I asked John, to comment on the TFCX in the context of the European and Japanese experiments.

Dr. DAVIDSON. I would comment on it in the following sense: The U.S. fusion program at the present time certainly has world leadership if you look at the content and the results coming from our facilities. If you look at the European program in particular, and take note of the capability of the jet device in the late eighties, I would say, without embarking on this next step experiment, the TFCX in the United States, there is a distinct risk of our losing our leadership, particularly to the Europeans, and perhaps to the Japanese.

Mrs. LLOYD. In referring to cost reductions, what cost reductions do you think could be realized if we located the TFCX at Princeton rather than at other fusion centers?

Dr. DAVIDSON. Making use of the existing facilities at Princeton, including buildings and power supplies and the like, the cost reduction would be in the range of $300-$400 million. It is relative to starting afresh at a new site, a yet undetermined site. It is also true that you could site the TFCX at some other center, some other major center, with existing power and facilities, and there would also be a cost reduction associated with that.

Mrs. LLOYD. How would we realize these cost reductions, more specifically?

Dr. DAVIDSON. Basically, the cost reduction that we were referring to was relative to starting——

Mrs. LLOYD. The power and the plumbing and the wiring and so forth?

Dr. DAVIDSON [continuing]. Up a brandnew site and laboratory for the TFCX.

Mrs. LLOYD. Do you think the CPMP is really written broadly enough to allow for program initiatives such as TFCX?

Dr. DAVIDSON. I think the CPMP, given an administration which projects flat budgets for fusion through fiscal 1988 or thereabouts, represents a reasonable plan insofar as it maintains program balance and allows, in later years, for initiation of a next major step in the program.

My main criticism of the plan relates to timing of taking such a step. The program is ready technically now to move ahead with an ignition experiment. We believe that that can be done in a relatively cost-effective manner, making use of slight increases in program resources, also making use of winding down of TFTR, for example. That would bring up some resources in the late eighties toward TFCX, and also making use of some of the development and technology program resources toward specific components for the TFCX. So it would require incremental funds, but there would also be some cost contribution from the existing base program toward the TFCX.

Mrs. LLOYD. Thank you very much.

Mr. Lujan?

Mr. LUJAN. Thank you, Madam Chairman.

I have always been concerned about the alternative concepts in fusion. My general feeling is that we're just not treating them quite fair, that somehow we are married to the big machines because that's the ongoing program and it continues. The loss in—Lawson—criteria, for example, in my opinion, judges all the different concepts, I suppose compares them to the big tokamaks.

Can you—are we giving those alternative concepts a fair shake? Are we looking at them in terms of "well, the big machines can do certain things," there is this concept, two or three ways of moving above in this particular concept? Are we judging those separate from the big tokamaks? Are they getting a fair shake? What kind of moneys are involved, what kind of a chance are we giving them to survive?

Dr. DAVIDSON. Let me first of all make a general remark. I think the U.S. program with regard to breadth of confinement concepts that it pursues—tokamaks, mirrors, reversed field pinches, compact toroids, stellarators—is unlike any other program in the world. We have a far broader and thereby intellectually richer fusion program than any other nation or collection of nations.

Now, the history of the fusion program in the past has been that when individual concept lines reach the point where there is technical merit for that concept line to proceed to the next level of experimentation, the program has taken that step. And it's true of tandem mirrors; it also is true of stellarators. I would simply assume that the program will continue to pursue that road in the future.

If you judge the program by what it has done in the past, we have followed up promising concepts to the next stage of development. Of course, it is difficult to do at the level you would like to in a flat budget.

Mr. LUJAN. What proportion of the moneys go to the high-density concepts?

Dr. DAVIDSON. I don't know those numbers. I would guess, in the area of experimentation, about—approximately $20–$25 million, including compact toroids and reversible pinches.

Mr. LUJAN. Out of what, 400 and——

Dr. DAVIDSON. 470, or thereabouts. Then there is additional theory and diagnostic development.

Mr. LUJAN. Are you satisfied that we're giving all alternative concepts a chance? I don't mean to push on one, but at one time we were talking about the rigatron that just seemed to me to be brushed off by the Department, of saying "we're just not interested in that", not even, it seemed to me at the time, a sufficent hearing. I'm not here just, you know, thumping for a particular project, but it just seemed to me like they were just waved aside and said well, that's——

To tell you the truth, what my impression was, was gee, if this works, then we have so many programs all over the country, that it would just really jeopardize the continuation of these experiments that we're doing. It seemed to me like it was viewed as a competition and just kind of waved aside.

Was my perspective correct on that, or——

Dr. DAVIDSON. As far as I understand, there has been no, in recent years, no proposal for constructing a rigatron ignition ex-

periment. But I would say that if you look at the activities in the federally funded fusion program, there certainly are design studies going on at the present time which look at high field reactor options of which rigatron is a special case in that spectrum.

To answer your earlier question, I believe that when a concept is ready for its next stage of experimentation, that proposals should be prepared and reviewed, and a good example of this perhaps is the reversed field pinch program at Los Alamos, which may be what you have in mind.

Mr. LUJAN. Yeah. OK. Thank you very much.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Lujan. Thank you very much, Ron, John. Thank you for your testimony.

Mrs. LLOYD. Our next witness is one of our good friends also, Dr. Leonard Reichle, who is chairman of our fusion advisory panel for our subcommittee. We welcome you. Thank you very much for all your good work. It's always good to see you with us.

STATEMENT OF LEONARD REICHLE, GROUP VICE PRESIDENT—ADVANCED TECHNOLOGY AND SPECIAL PROJECTS GROUP—AND MEMBER, BOARD OF DIRECTORS, EBASCO SERVICES INC.

Dr. REICHLE. Thank you, Madam Chairman.

Madam Chairman and members of the subcommittee, my name is Len Reichle. I am group vice president and member of the board of directors of Ebasco Services Inc. Ebasco is the industrial subcontractor of the Princeton Plasma Physics Labortory and is responsible for the engineering, design, procurement, installation and assembly of the TFTR. As you know, our work was completed on schedule, thereby enabling Princeton to achieve "first hydrogen plasma" on December 24, 1982.

You have asked me to comment on the Department of Energy's comprehensive program management plan. I am going to try to be constructively critical and, therefore, lest there be any mistake in interpreting what I say, I would like to commend John Clarke, the directors and members of the staffs of the national laboratories and universities, for what I think is a really excellent job in advancing our national fusion energy program as it has been advanced.

However, I am going to comment first as chairman of the subcommittee's panel on fusion energy, and next I can't help but have in mind also the role I have had in connection with the TFTR, and also as someone who for more than 25 years has been a consultant to Government and industry on energy.

Mrs. LLOYD. Excuse me, Dr. Reichle. We do have a vote, so before you get into your testimony, I will make the vote and we'll stand in recess for the vote.

[Whereupon, the subcommittee was in recess.]

Mrs. LLOYD. We will come to order.

We do apologize for the delay, and please proceed with your testimony, Dr. Reichle.

Dr. REICHLE. Thank you.

You asked for my comments on six topics with regard to the plan. Here are my comments.

First, topic one—the CPMP as a whole. The CPMP is not a plan directed toward the implementation of the Magnetic Fusion Engineering Act of 1980, as I believe Congress intended it to be. Instead, it is a description of what DOE proposed in June 1983 to do under the budgetary limitations imposed by the administration. It is too little and it may jeopardize our national energy security by making us too late in having commercial fusion electric power available when it may be needed for an energy crisis in the future.

Committee topic No. two—strengths and Weaknesses of the CPMP. I raise five questions.

My question No. one—does the CPMP recognize the value and urgency of fusion energy? The executive summary of the CPMP states in part:

> The Federal Government support of the fusion program is predicated upon the need for the fusion energy option in the Nation's energy future . . . As a long-term energy option, magnetic fusion energy has characteristics which allow it to become a principal energy source in the next century. The attractive characteristics include an inexpensive, vast and secure fuel reserve, as well as environmental and safety features that appear to be desirable. . . .
>
> In the near-term, there is also a benefit from high technology development flowing from the fusion program.

In view of this expression of the potential of fusion energy, one might expect a vigorous program commensurate in content and schedule with such an affirmative assessment. Instead, the CPMP is a weak stretch-out of a science-oriented program without an engineering oriented next project to focus effort and to set priorities for technology development. And, it falls short of meaningful industrial participation—at a time when the competing Japanese program is solidly entrusted to Japanese industrial management and technical direction.

Committee members: In my opinion, and in the opinion of many scientists, engineers, and businessmen, accelerated development of fusion energy for electric power generation is of vital importance to our national energy security. The current oil glut appears to be temporary. Another energy crisis is a distinct possibility and perhaps a probability. Conflicts in the Middle East or foreign national policy may again stop the flow of oil from Saudi Arabia. Energy independence is again diminishing. In 1977, we imported a total of 8.8 million barrels of oil per day. This dropped to 2.8 million barrels per day, but is now back to 5.3 million barrels per day. Our own oil reserves are limited, and U.S. oil consumption again exceeds new discoveries.

Fragile public acceptance of nuclear power and other factors have severely restricted the use of light water reactors. Breeder reactors are likely to experience even more difficulty. Coal-fired powerplants are beginning to experience serious regulatory concerns, particularly over SO_2 and NO_X releases, often associated with acid rain. Solar, OTEC, fuel cells, and other alternate energy systems are either limited in their application or limited in their potential.

Only fusion energy promises to produce from essentially limitless fuel resources, abundant energy that is low cost, safe, environmentally compatible and publicly acceptable. This is an enormous potential which justifies a much higher sense of urgency than is ac-

corded it by the CPMP. Fusion energy's promise justifies a higher national priority.

My question No. 2, regarding strengths and weaknesses: Does the CPMP identify a next project which in the near future can be engineered and built to achieve a next-step objective?

The most desirable next step objective—after breakeven in the TFTR in about 1986—is to demonstrate ignition and long-pulse equilibrium burn. This requires a new fusion device. The CPMP does not include in its program a next project directed toward achieving the next-step objective in the near future.

My question No. 3, regarding strengths and weaknesses: Does the CPMP provide for effective international leadership and cooperation?

The recommendations of the Foster Committee and of the Buchsbaum Committee, and recent magnetic confinement technical accomplishments, indicate that the U.S. fusion energy program is ready for engineering development. The CPMP proposes to delay engineering development until after completion of a further research and technology development program. This is not a desirable course of action.

We are now destined to lose our leadership in the development of fusion energy to Western Europe and Japan in the late 1980's. The CPMP, while it states an intention of maintaining world leadership, does not provide a realistic path for recapturing the lead in the 1990's. A next project fusion device in the United States, which the CPMP does not provide for, would serve to focus engineering and technology development and regain leadership.

My fourth question, regarding strengths and weaknesses: Is the competition between tokamaks and mirrors proposed in the CPMP sound?

The CPMP proposed competition between the tokamak and the mirror concepts to produce what is referred to as "product definition," that is, selection of one over the other, is ill conceived. Both the toroidal and mirror programs make important contributions to the U.S. magnetic fusion program. Both should be pursued vigorously, with the toroidal concept in the lead role under the program direction of the Princeton Plasma Physics Laboratory and the mirror concept in a support role under the program direction of Lawrence Livermore.

My fifth question, regarding strengths and weaknesses: Does the CPMP propose the most meaningful industrial participation?

The most meaningful industrial involvement in the fusion energy program is essential if we are to make significant strides ahead and recapture international leadership. How does the CPMP propose to achieve this? It says:

> The system studies work to develop conceptual reactor designs for the leading concepts, as well as system studies necessary to prepare for the selection between long-pulse tokamaks and tandem mirrors, will be performed as a joint effort of national laboratory teams and the Fusion Engineering Design Center [FEDC], which is an industrially based central engineering design resource.

This is good, but not the most significant industrial involvement. Assignment of a few people to work under the supervision of FEDC is not the most meaningful industrial involvement. The most meaningful industrial involvement is obtained when the Government or

the laboratory contracts for the industrial company to be responsible for managing and technically directing a scope of work to be performed. This is how PPPL handled the TFTR project. Princeton contracted with Ebasco, and Ebasco, with Grumman Aerospace as its project team subcontractor, performed the engineering, design, procurement, installation and assembly of the TFTR. This is how DOE planned to handle the EBT-P project at Oak Ridge, with Union Carbide selecting McDonnell Douglas as the contractor to perform such functions.

Design engineering is one of industry's greatest strengths. Yet, the CPMP assigns this function to a "design center" at a national laboratory. Design engineering should be assigned to an industrial subcontractor of the lead laboratories—Princeton for tokamaks and Lawrence Livermore for mirrors.

Committee topic three—major milestones of the CPMP. Under the CPMP, the entire period between now and year 2000 would be devoted to the development of base science and engineering technology, without a "next project" around which technology can be oriented. Our national best interest requires more than to wait that long for a next project.

Committee topic four—adequacy of funding levels for the CPMP. On funding, the CPMP states, "The annual budget required * * * is judged to be at about the current program level, after correcting for inflation, for each of the 5 years beginning in the fiscal year 1984."

The current annual level of funding of the U.S. fusion energy program, about $470 million, is clearly inadequate. The fusion energy budget of the United States should be increased to at least $500 million in fiscal year 1985 and thereafter ramped up to a level of approximately 25 to 50 percent above the present level in constant dollars. This would provide base funding for the continuation of a program of research and associated technology development and provide incremental additional funding for promptly supporting the design of a next project fusion device. This is imperative if we hope to recapture world leadership and to have fusion energy developed to the point where we are prepared quickly to make fusion a large-scale producer of electric energy should an emergency exist in the future.

Committee topic five—level of engineering development needed.

What is needed is a "next project" to achieve both the physics objectives of ignition and long-pulse equilibrium burn and to focus the development of technology on a project. We learn by doing the systems engineering, the trade-off studies, the conceptual design, and detailed design on a "next project." We learn by the engineering of supporting equipment, systems and structures on a project. We learn by having design engineers work with manufacturers and fabricators of components on a project. We learn by installing and assembling a project.

The level of engineering development needed is what it takes to produce the "next project" fusion device in a technically strong and cost-effective manner, that is, with the laboratories and universities doing what they can do best, such as physics, research, and overall program management, and with industry doing what it can do best, such as engineering design, engineering development,

project management, procurement, construction, installation and assembly, and physical operation and maintenance.

Finally, committee topic six—the fusion community's latest thinking on upgrades of TFTR and MTFF-B.

The fusion community's latest thinking is, I believe, that we need, in addition to a vigorous base program, a "next project" which should be undertaken promptly, and that such a "next project" should be a tokamak fusion core experiment, a TFCX. This would be a copper coil device based on TFTR experience. Planning and preliminary engineering would start in fiscal year 1984. It would be about a 7-year effort, with a PACE cost of approximately $1 billion. Princeton would have technical responsibility for the project and be project leader. Princeton would subcontract to industry for project management, design engineering, procurement, construction, installation, final acceptance testing, and some operational responsibility. The MFTF-B upgrade would be used for steady-state testing of blanket and power system components complimentary to the TRCX. Also, I find strong support exists for proceeding with the Mike McCormack Fusion Materials Irradiation Test Facility. Such a facility would contribute importantly to our knowledge of the materials needed in fusion machines.

Thank you.

[The prepared statement of Dr. Reichle follows:]

TESTIMONY

to the

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

of the

COMMITTEE ON SCIENCE AND TECHNOLOGY

of the

U. S. HOUSE OF REPRESENTATIVES

by

Leonard F C Reichle, Group V-P
Ebasco Services Incorporated,
an ENSERCH Corporation
October 20, 1983

Madame Chairwoman and Members of the Subcommittee:

My name is Len Reichle. I am Group Vice-President, (Advanced Technology and Special Projects Group) and a member of the Board of Directors of Ebasco Services Incorporated. Ebasco is the Industrial Subcontractor of the Princeton Plasma Physics Laboratory (PPPL) and is responsible for the engineering, design, procurement, installation and assembly of the Tokamak Fusion Test Reactor (TFTR). As Ebasco's Responsible Corporate Officer, I directed the TFTR staff of Ebasco, of our Project Team subcontractor (Grumman Aerospace) and of more than 400 other Ebasco subcontractors. I am happy to report that our work was completed on schedule, thereby enabling PPPL to achieve "first hydrogen plasma" on December 24, 1982.

My comments on the Department of Energy's Comprehensive Program Management Plan for Fusion Energy (CPMP) are based on my experience as:

Chairman during the past two years of your Subcommittee's Panel on Fusion Energy.

Someone who was deeply involved in a responsible manner with designing and building the world's largest operating tokamak.

Someone who, for more than 25 years, has been a consultant to government and industry on energy - from the design and construction of electric generating stations, to the assessment of energy supply and demand and to the risks of energy shortfall.

My comments on the six Topics identified in the invitation for me to speak on the CPMP are the following:

1 - The CPMP as a Whole

The CPMP is not a Plan directed toward the implementation of the Magnetic Fusion Engineering Act of 1980, as I believe Congress intended it to be. Instead, it is only a description of what the U S Department of Energy (DOE) proposes to do under the budgetary limitations imposed by the Administration. It is "too little," and it may jeopardize our national energy security by making us "too late" in having commercial fusion electric power available when it may be needed for an energy crisis in the future.

2 - Strengths and Weaknesses of the CPMP

a - Does the CPMP Recognize the Value and Urgency of Fusion Energy

The executive summary of the CPMP states in part:

> "The Federal Government support of the Fusion program is predicated upon the need for the fusion energy option in the nation's energy future.... as a long-term energy option, magnetic fusion energy has characteristics which allow it to become a principal energy source in the next century. The attractive characteristics include an inexpensive, vast and secure fuel reserve, as well as environmental and safety features that appear to be desirable....
>
> "In the near-term, there is also a benefit from high technology development flowing from the fusion program. Fusion program development fosters advances

> in technology areas as diverse as superconducting magnets, high power accelerators and radio frequency systems, advanced materials development, and mathematical methods for computers. Fusion research also makes important contributions advancing electronic, computer controlled systems, optical measurement and in many other areas." (p. 1)

In view of this expression of the potential of fusion energy, one might expect a vigorous program commensurate in content and schedule with such an affirmative assessment. Instead, the CPMP is a weak stretch-out of a science oriented program without an engineering-oriented "Next Project" to focus effort and to set priorities for technology development. And, it falls short of meaningful industrial participation (at a time when the competing Japanese program is solidly entrusted to Japanese industrial management and technical direction).

Committee members: in my opinion and in the opinion of many scientists, engineers and businessmen, accelerated development of fusion energy for electric power generation is of vital importance to our national energy security! The current oil glut is temporary. Another energy crisis is highly likely. Conflicts in the Middle East or foreign national policy may again stop the flow of oil from Saudi Arabia. Energy independence is again diminishing. In 1977, we imported a total of 8.8 million barrels of oil per day. This dropped to 2.8 million barrels per day but is now back to 5.3 million barrels per day. Our own oil reserves are limited, and U. S. oil consumption again exceeds new discoveries.

Fragile public acceptance of nuclear power and other factors have severely restricted the use of Light Water Reactors. Breeder Reactors are likely to have even more difficulty. The phenomenon of "acid rain" hangs like a foreboding cloud over coal-fired electric generation. Solar, OTEC, Fuel Cells and other alternate energy systems are either limited in their application or limited in their potential.

Only Fusion Energy promises to produce, from essentially limitless fuel resources, abundant energy that is low-cost, safe, environmentally compatible and publicly acceptable. This is an enormous potential which justifies a much higher sense of urgency than is accorded it by CPMP. Fusion Energy's promise justifies a higher national priority.

b - Does the CPMP Identify a "Next Project" Which in the Near Future Can Be Engineered and Built to Achieve a "Next-Step" Objective?

The most desirable "Next-Step" objective (after breakeven in the TFTR in 1986) is to demonstrate ignition and long-pulse equilibrium burn. This requires a new fusion device. The CPMP does not include in its program a "Next Project" directed towards achieving the "Next-Step" objective in the near future.

c - Does the CPMP Provide For Effective International Leadership and Cooperation?

The CPMP states:

> "The U. S. has attained a leadership role in world-wide fusion activities....the Department intends

to maintain this position...." (p. 4)

But, again, the CPMP fails to propose what is needed to sustain a role of international leadership.

Recommendations of the Foster Committee and of the Buchsbaum Committee and recent magnetic confinement technical accomplishments indicate that the U. S. Fusion Energy Program is ready for engineering development. The CPMP proposes to delay engineering development until after completion of a further research and technology development program. This is not a desirable course of action.

We now are destined to lose our leadership in the development of fusion energy to Western Europe and Japan in the late 1980's. The CPMP, while it states an intention of maintaining world leadership, does not provide a realistic path for recapturing the lead in the 1990's. A "Next Project" Fusion Device in the U. S. will serve to focus engineering and technology development.

d - Is the Competition Between Tokamaks and Mirrors Proposed By the CPMP Sound?

The CPMP proposed competition between the Tokamak and the Mirror Concept to produce what is referred to as "Product Definition," i. e. selection of one over the other, is ill-conceived. Both the toroidal and mirror program make important contributions to the U. S. magnetic fusion program. Both should be pursued vigorously, with the Toroidal concept in the lead role under the program direction of PPPL and the Mirror concept in a support role under the program direction of the Lawrence Livermore National Laboratory (LLNL).

e - Does the CPMP Propose Meaningful Industrial Participation?

The CPMP states:

> "We plan to maintain the substantial industrial participation already in place in the next phase of the program." (p. 4)

Real and meaningful industrial involvement in the fusion energy program is essential if we are to make significant strides ahead and recapture international leadership. How does the CPMP propose to achieve this? It says:

> "The system studies work to develop conceptual reactor designs for the leading concepts, as well as system studies necessary to prepare for the selection between long-pulse tokamaks and tandem mirrors, will be performed as a joint effort of national laboratory teams and the Fusion Engineering Design Center (FEDC), which is an industrially based, central engineering design resource at the Oak Ridge National Laboratory (ORNL). The Department will also use an industrial contractor to establish evaluation criteria and normalizing standards, and then assess the design studies ...." (p. 4)

This is no significant industrial involvement! To say that the Fusion Engineering Design Center at ORNL is "industrially based" is a play on words. Assignment of a few people to work at Oak Ridge under the supervision of FEDC is not meaningful industrial involvement. Meaningful industrial involvement is obtained when if the government or the laboratory contracts for the industrial company to be responsible for the scope of work to be performed i. e. to

perform (under the overall program direction and financial control of the Government or Laboratory) the project management and technical direction of industrial company engineers, scientists, designers, estimators, construction and installation staff, and others working toward performance and schedule objectives. This is how PPPL handled the TFTR Project. PPPL contracted with Ebasco, and Ebasco (with Grumman Aerospace as its Project Team subcontractor) performed engineering, design, procurement, installation and assembly of the TFTR. This is how DOE planned to handle the EBT-P Project at Oak Ridge, UCCND selecting McDonnell Douglas as the contractor to perform such functions.

Design engineering is one of industry's greatest strengths. Yet, the CPMP assigns this function to a "Design Center" at a National Laboratory. Design engineering should be assigned to an industrial subcontractor of the appropriate Lead Laboratory (PPPL for tokamaks and LLNL for mirrors).

3 - Major Milestones of the CPMP

Under the CPMP, the entire period between now and Year 2000 would be devoted to the development of base science and engineering technology, without a "Next Project" around which technology can be oriented. It is not in our national interest to wait that long.

4 - Adequacy of Funding Levels for the CPMP

On funding the CPMP states:

> "The annual budget required...is judged to be at about the current program level, after correcting for inflation, for each of the five years beginning

> in the fiscal year 1984.... a decision to proceed... on an engineering device would require increasing budget in subsequent years." (p. 19)

The current level of funding of the U S Fusion Energy Program (about $470 million) clearly is inadequate. To recapture world leadership in the 1990's and to assure timely development of what gives promise of becoming one of our major sources of commercial electric energy, the Fusion Energy budget of the U. S. should be increased to at least $500 million in FY 1985 and thereafter ramped up to a level of approximately 25-50 per cent above the present level in constant dollars. This would provide "base" funding for the continuation of a program of research and associated technology development and provide "incremental" additional funding for promptly supporting the design of a "Next Project" Fusion Device. This is imperative if we hope to recapture world leadership and to have fusion energy developed to the point where we are prepared quickly to make Fusion a large-scale producer of electric energy should an emergency in the future require it.

5 - Level of Engineering Development Needed.

What is needed is a "Next Project" to achieve both the physics objectives of ignition and long-pulse equilibrium burn and to focus the development of technology on a project. We learn by doing the systems engineering, the trade-off studies, the conceptual and detailed design of a "Next Project." We learn by the engineering of supporting equipment, systems and structures. We learn by having design engineers of industry work with manufacturers and fabricators of components. We learn by installing and assembling the "Next Project."

The level of engineering development needed is what it takes to produce the "Next Project" Fusion Device in a technically strong and cost-effective manner i. e. with the laboratories and universities doing what they can do best (physics, research and over-all program management) and with industry doing what it can do best (engineering design, engineering development, project management, procurement, construction, installation and assembly, and physical operation and maintenance).

6 - The Fusion Community's Latest Thinking on Upgrades of TFTR and MFTF

The Fusion Community's latest thinking is, I believe, that we need, in addition to a vigorous "base" program, a "Next Project" which should be undertaken promptly, and that such a "Next Project" should be a Tokamak Fusion Core Experiment (TFCX). This would be a copper coil device based on TFTR experience. Planning and preliminary engineering would start in FY 1984. It would be a seven year effort, with a PACE cost of approximately $1 billion. PPPL would have technical responsibility for the project and be Project Leader. PPPL would subcontract to industry for project management, design engineering, procurement, construction, installation, final acceptance testing, and some operational responsibility. The MFTF Upgrade would be used for steady-state testing of blanket and power system components complimentary to the TFCX. There is strong support for firming up an international cooperative arrangement for proceeding with the Mike McCormack Fusion Materials Irradiation Test Facility, which will contribute importantly to our knowledge of the materials that can be used most effectively in fusion machines.

Mrs. LLOYD. Thank you very much, Len, for not only your outstanding statement but for the work and the dedication to our fusion program. We certainly appreciate all your efforts.

What priorities really would your panel assign to the FMIT facility, fusion materials to the test facility?

Dr. REICHLE. I think the answer to that probably, if I can sense the feeling of the committee members, is that first we need the base program, next we need a project around which future progress can be oriented, a TFCX. The FMIT really would be part of the base technology program, but it would be a facility that would tie in both with the fundamental work being done in the field and also serve the ends of the specific project, the TFCX also.

Mrs. LLOYD. You mentioned the Mike McCormack facility. I was wondering if the EBTP might be a good facility for studying candidate materials that would withstand high energy neutrons.

Dr. REICHLE. EBTP was not designed for that purpose and probably would not be as good as a specific facility designed to achieve materials evaluation ends.

Mrs. LLOYD. So you do not recommend the EBTP could play a role in the study?

Dr. REICHLE. Not a role in the sense of a materials testing facility.

Mrs. LLOYD. What role do you see that it could play?

Dr. REICHLE. I would think it is another option similar to a fusion TFCX or a mirror machine, if there is funding enough to go around to really support effectively another project of that kind. The problem always is that there are not enough funds to go around and I think it drops down in priority in the general view of the panel below a TFCX and below the support that the alternate concept mirror might provide.

Mrs. LLOYD. For the Energy Research Advisory Board, Dr. William E. Drummond's prepared statement mentioned that in assessing the current status and direction of the fusion program, we must clearly distinguish between program plans which seeks to produce fusion power most expeditiously and program plans which seek to realize the fullest possible potential for the fusion process.

What is your basic impression then of this relationship of the CPMP? Does it really take the proper course for the program?

Dr. REICHLE. No, as I mentioned in my testimony, I believe if any of us redid the CPMP today, it would be done slightly differently. And I think the principal change that needs to be made from the CPMP is to get orientation around a "next project", because we always learn so much more if all of our technology development work is oriented around a specific project. So that's the first thing.

Next I think we can improve the quality and degree of industrial involvement in the future of the program over what we have had in the past. I think we have made some very great strides in that, such as on the TFTR, and I think if we do more and more of this, as was going to be the case with McDonnell Douglas doing the EBTP, we would have a stronger total program.

Mrs. LLOYD. Of course, you said that certainly our budget is not adequate. But would you comment, in view of the statement that you have made, comment on the FMAC's recommendation to the

TFCX budget enhancement and that they are recommending an increase in the program enhancement.

Dr. REICHLE. I agree fully that there should be that order of magnitude enhancement. I recognize that we're all living in a period of budgetary restraint, so what we're talking about in the end is whether this is of higher priority than some other program, perhaps even outside of the energy field.

My own feeling is that energy is one of the major factors that propels an industrial economy, and while we're in a period of lull now and people are not aware of an energy crisis any more, as they were a few years ago, such a crisis can reappear very quickly. As we all know, we're having great difficulties with water reactors now, with the breeder reactor in terms of public acceptance, coal is having all kinds of difficulties in terms of SO_2 and NO_x and public acceptance. Here is a source of energy that is clean, publicly acceptable, environmentally very compatible, with limitless energy resources to work from.

So I would urge consideration of trying to provide additional funding for the fusion program, even if it has to come from outside the Department of Energy program, just because of the critical and basic importance of energy to our whole present manner of living and to our international competitive position. Energy is a very, very important thing in the world today.

Mrs. LLOYD. We certainly agree with your philosophy.

Mr. Lujan?

Mr. LUJAN. Thank you, Madam Chairman. And while that makes a lot of sense, fusion is very acceptable now in all areas because it's a long ways away. When we get there, we'll probably have the same problems that we have with all other forms of energy that we're having now.

Some time ago the Fusion Advisory Committee had recommended—and I'm pulling some figures out of my head; this has been about 3 or 4 years ago—that at that time we were at a funding level of $300 million a year or somewhere in there, and that at that rate we would be able to generate electricity by the year 2010, 2020. And we were told at that time that if we moved it up to about $450 million—and we're at about that level now, $470 million I believe it is—that then that would push it up to perhaps the year 2000, and that if we really wanted fusion generated electricity, that if we went up to $750 million annually, that we might be looking at a 1990 date.

I like that concept, because it said, you know, if you put so much money in, you're going to be able to—the more money you put in, the closer you get to it.

Do those figures still hold today, or does that concept still hold today, that maybe now it would be 1992 rather than 1990, for $750 million?

Dr. REICHLE. At one time there was a rough plan for proceeding from the TFTR, which is in operation now, to an intermediate project that would come into operation about 1989 or 1990, and then a demonstration plant that might come into operation about the year 2000, within some numbers as you have mentioned. What we have done as a nation is not to authorize those additional steps but rather to broaden the base of our total technology program. So

I think we're sometimes in the situation where, when given a choice, you want to take both. We have done that a little bit. We have broadened our total base, and there is great merit in that in terms of backup strength and overall technological support.

But I think we need to narrow in now and get this TFCX on the line and then get the next stage, and I think having an objective like electric power generation, rather than just achieving the broad potential of fusion, gives a very specific target to work toward. I think within any budget level like the 350-, or 500- to 750-, you can achieve these many different things, but we ought to focus in on a target like fusion electric power and then do the things that are necessary to achieve that end with as much back-up support as we can afford as a nation to put in the total fusion program.

Mr. LUJAN. Well, you have recommended, in essence, as I was looking at your percentages, the same thing, that next year maybe up to $500 million, and then after that you say 25 to 50 percent increase, which would take us to $700 or $750 million.

Do you think that will bring closer the date when we can have electrical generation by fusion?

Dr. REICHLE. Well——

Mr. LUJAN. Or is that just going to keep up our same level?

Dr. REICHLE. No. We have been involved now in the engineering of TFTR for more than 7 years, with a peak staff of over 500 people involved at one point. We have worked very, very closely with and under the overall direction of the Princeton Plasma Physics Laboratory, and for our part I can say that we are optimistic, based on, one, the technical accomplishments on the physics side of the tokamak magnetic confinement program, and on the engineering accomplishments that we and the 400 contracts on a subcontract basis with us have achieved. So we think it is, indeed, a doable thing and that the promise of fusion electric energy can be made a reality.

Mr. LUJAN. Didn't MIT tell us they could do it in their—you know, without having to do this big $2 billion machine—what did we call it then, the FEB, the Fusion Engineering Building or something—I can't remember what it was. Anyway, it's kind of the Mike McCormack thing over——

Mrs. LLOYD. FED.

Mr. LUJAN. FEB?

Mrs. LLOYD. FED.

Mr. LUJAN. OFED. Didn't at that time MIT come and say "Hey, we can achieve ignition and length of time without your going into that big expensive program." Did they tell us that, Jack? Do you remember that?

Anyway, whatever, I thought they had told us that.

We still get back to the same old trail that I've been following all along, I guess, because you keep talking about tokamaks and mirrors and very little look is taken at other alternative concepts.

Why do we keep doing that? Why—you know, I remember at one time that this machine was proposed, and the thought was that it was a smaller machine and that the plasma would go around faster and naturally it would wear out the sides of the toriod, but then you'd just replace that and put in another one. The beginning costs, we were looking at that time for about $7 or $8 million, I

can't remember what it was, with the eventual cost being in the area of $50 or $100 million to build this machine, rather than the big expensive amounts, and that the maintenance on it, because you would take out the hole in the doughnut, so to speak, and throw it away, for the $1 million it would cost you to put in that new hole in the doughnut.

No one never said why it was not any good. They just said well, that's—they didn't quite say it was a hairbrained idea, but intimated that. Have you looked at something like that and see if as a matter of fact it——

Dr. REICHLE. I would like to address I think the overall intent of your comment first, if I've interpreted it correctly, and then get back to that specific question.

I think in any research and development program, whether it's General Motors or IBM or, say, the fusion program, what people try to do is pick the highest probability of success project, which I think by general consensus is the magnetic fusion TFTR type approach. Then you try to pick your next most likely to succeed runner-up, which by general consensus of the fusion community is the mirror machine. You fund the number one with as much as you can, as a front-runner, and the second amount of funding comes to the backup, and then beyond that you provide some funding to several other probjects, such as Bob Bousard's machine, such as the sphereamac at Princeton and others. So I don't think, as I have seen it, there is any intent to squash any of these other potentials and to hold them back, but on the basis of probability of success, by consensus of the fusion community, money has gone, and I think probably so, to the tokamaks as the front-runners and then to the mirrors as the backups, then with additional funding for these other approaches.

Mr. LUJAN. But isn't it worth the roll of the dice—and my constituents would die if we're going to—you know, that I'm proposing that we roll the dice for $7 or $8 million to try something out. But when you look at $470 million, it's not a bad deal to roll the dice for $6 or $7 million or $8 million, to just take a look at it. Don't you think it's worth a——

Dr. REICHLE. I think we are doing that on some of these backup machines, such as the machine at Los Alamos——

Mr. LUJAN. Oh, I'm not very pleased with that one.

Dr. REICHLE [continuing]. Which is one of these backup machines, and there are several others that are being funded, one at Mathematical Sciences now, the sphereamac. So I think we should try to do both. It's just a matter of where you put the greater emphasis and how much money you can provide to the others.

Now, if we get a significantly expanded fusion program, total fusion program, then perhaps more money can be applied to some of these third echelon backup machines.

Mr. LUJAN. Thank you.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Lujan.

Thank you very much, Dr. Reichle.

Dr. REICHLE. You're welcome.

Mrs. LLOYD. We appreciate all of your good help.

Mrs. LLOYD. Our next witness is Dr. Stephen Dean, president of Fusion Power Associates. We welcome you and we look forward to your testimony.

STATEMENT OF STEPHEN O. DEAN, PRESIDENT, FUSION POWER ASSOCIATES

Dr. DEAN. Thank you, Madam Chairman. It's a pleasure to be back.

It's hard for me to believe that it's been 3 years since we passed the Magnetic Fusion Act. It's been a very frustrating 3 years for us fusion advocates, including, I know, members of this committee and many Members of Congress. It's been doubly frustrating for me, having spent 25 years of my life trying to work the fusion problem and to be here talking about what appears to me to be questions of initiating things which we all agreed to initiate 3 years ago, and which in many ways the fusion community tried to initiate a couple of years before that. I believe we have been ready to initiate the next step in the tokamak program since about 1978, after we had, as you recall, the great results from the Princeton PLT tokamak which essentially guaranteed us that, with proper development, we could ignite and burn and make net power using the tokamak concept. In many ways, if you think about it, if we had embarked on the fusion engineering device at a cost of $1 billion that was advocated and legislated by the Congress in the Fusion Act, we would have it half built by now. And yet here we are, sitting and talking about committees which are still meeting and essentially recommending to us the same thing.

The name of the machine has changed; the cost is still $1 billion. Maybe we're getting a little smarter and maybe we can make it for a little bit less, but to make the kind of power and handle the power that we're talking about for a sustained fusion reaction, it's not going to come for $5 or $10 million. We're going to have to spend a good fraction of $1 billion to build that machine. And until we've done that, I don't think we're going to have commercial credibility in this program.

I had the good fortune to have worked at ERDA and AEC in the seventies with Bob Hirsch, and in 1976 I chaired under Bob's direction for fusion a preparation of a five-volume comprehensive program management plan, much more comprehensive than the program plan you have in front of you. It was a five-volume set which showed you several options and logics, costed out the prices of the facilities and steps that we needed, as best you could tell, and came up with the figure that for about $15 billion, what it was going to cost, you could have fusion anywhere from like the midnineties to slightly after the year 2000.

Now, the key result from that study was not that the program needed $500 or $700 million a year. It really isn't the exact level of effort in the program that matters. The key thing that determines when you have fusion is when you initiate successive stages of devices. It's the devices that pace the program. It's the devices which move accomplishment forward and develop the engineering and scientific credibility in the program. It's devices that take 6, 7 years to build. And it's because of that that when you initiate the devices

is what determines when you get to the end point. So as long as we don't initiate the device, the next device, you can spend $500 million a year from now until 3000 and you're not going to make a fusion reactor by developing more scientific understanding of the concepts. You may get smarter and do a lot of good science, but you're not going to make electricity using fusion until you start initiating a series of more ambitious, larger, more expensive machines.

This then has been the issue since 1978. It was the issue that was recognized in the Fusion Act, that we must initiate the fusion engineering device, and after that we must build a powerplant. It's this that the administration does not want to do. It's not really a question so much of the flat budget, although that is certainly a major part of the problem. But the real problem is that the administration does not want to initiate a next generation expensive machine.

Now, the corollary of initiating such a machine is, in fact, some increase in the budget, because the other thing that we showed in our 1976 plan was that these machines are so expensive it's not reasonable to think that you can pay for these construction projects out of the operating program.

There used to be in the AEC days a very clear distinction in the Congress and in the administration between the operating budget and the construction budget. We recognized that some level of operating budget, cost-of-living increases or whatever, was required for a stable program. But the other thing that was always recognized was that if you're going to build a big project, you've got to pay for it. You can't absorb the cost of a billion dollar machine in an operating program, even if that program is running at $400 or $500 million a year operating. You must, when you decide to build the machine, you must agree to pay for it.

And that, I think, is one of the problems that John Clarke has had to face in trying to prepare this comprehensive program management plan. How can he initiate or propose to initiate a next generation machine if he's told that the budget is flat? I don't have any objection to having a flat or relatively flat operating budget. What I do object to is the unwillingness to say, if we're going to build a machine, a construction project, we must add on for that period of time the cost required to build that machine.

So I think that is the issue that we keep coming back to in the program. We must initiate the next generation machine if we're ever to get to practical fusion power, and when we do initiate it, we must be committed to pay the incremental costs of that machine.

Mrs. LLOYD. Do you feel we shouldn't in our proposals, in our authorization bill, that we should line item——

Dr. DEAN. Beg your pardon?

Mrs. LLOYD. A more of a line item form——

Dr. DEAN. I think we must have a line item for this machine, and when we put the line item in, we must be committed to pay the cost of that machine as an increment on the total budget, for whatever period of time it takes to construct that project. We cannot pay for a construction project out of the base, so-called base program. We can contribute to it perhaps; you can take some of the old construction money that was previously spent on a different machine and roll it over, but because these machines tend to get

more expensive each time, you can't pay for them by taking money out of previous machines.

We went through this on TFTR. The question was, can we build a $300 million TFTR by shutting down the PLT's and the Doublet III's. If we shut them all down, we wouldn't have had enough money to pay for the TFTR, because it's moving into a next generation technology, it's more expensive, and therefore you don't have enough money in the previous machines, which only cost $30 million each, to pay for a $300 million machine. And so if you're going to build a next generation machine that makes power, which you eventually have to if you have a practical motivation, you must pay some incremental contribution to this as a line item construction project for that period of time that it takes to build the device.

That doesn't mean that you're committed to running a billion dollar a year total program forever. It means that you might have some level of operating budget you've agreed to and then, as a delta, as an add-on, during construction you pay for that machine separately.

To get back to my prepared testimony, looking into these kinds of issues and the problems of not implementing the Fusion Act, the board of directors of Fusion Power Associates, which is an association composed of 50 corporate members and affiliates, we prepared a new policy statement that we believed was appropriate for today. I would like to insert the full text of that into the record.

Mrs. LLOYD. Without objection, it will be made a part of the record.

[The material referred to follows:]

FUSION POWER ASSOCIATES
POLICY STATEMENT ON FUSION ENERGY DEVELOPMENT 1983

The Board of Directors of Fusion Power Associates, on behalf of the association, urges that fusion energy development be based upon the following

RECOMMENDED POLICIES

- The expeditious development of practical fusion power should receive high national and international priority.
- The engineering aspects of fusion development require substantial additional effort now.
- Commitments to new and improved experimental facilities are urgently needed to insure continued program momentum and progress into the 1990's.

The above recommended policies are based upon the following

FINDINGS AND OPINIONS

- We reaffirm our conviction that the principles and policies established by law in the Magnetic Fusion Energy Engineering Act of 1980 provide a sound basis for the expeditious development of practical fusion power.
- Scientific progress in fusion research gives increasing evidence that this new energy source will emerge early in the twenty-first century. There very likely will be a variety of paths to practical fusion power.
- It would be premature, at this stage, to judge which of the variety of magnetic and inertial fusion concepts will ultimately succeed commercially. This fact should not discourage use of the best available concepts in the design and construction of near-term fusion test facilities.
- The fusion programs in the U.S., U.S.S.R., Europe and Japan have comparable accomplishments, facilities and momentum. The present rate of progress in the world program is based on capital investment commitments made in the 1970's. Progress will predictably diminish unless new facilities are commissioned soon.
- International cooperative agreements have been a substantial factor in fusion progress and should be encouraged. Such agreements, however, are not an effective substitute for focused national efforts, national experimental facilities and test reactors.
- Governments should encourage and promote the full participation of industry in the planning, research and development as well as in the engineering and operating aspects of the fusion programs. They should also strengthen and encourage fusion-related programs in universities to ensure an adequate supply of engineers and physicists with knowledge in this field.
- The pace of fusion energy development today is clearly limited by the funds available rather than by science and technology. The skills required for engineering development exist today in a variety of institutions — industry, national laboratories, and universities. If these skills are properly mobilized and utilized, the time will soon come when society may reap the considerable benefits which fusion has to offer. These benefits include a virtually inexhaustible fuel supply, readily available to all nations.
- The availability and affordability of energy are essential elements to healthy and vigorous industrial society and are necessities for developing nations. Assured energy supply is an important determinant of world peace and security and a requirement for progress of future generations.
- We commend the governments of the world for making a sustained investment in fusion research. We believe the time has come to begin to capitalize on this investment by placing increased emphasis during the 1980's on the engineering and systems design aspects of practical fusion energy systems.

Fusion Power Associates is a non-profit, tax-exempt corporation established to ensure the timely development and acceptance of fusion as a socially, environmentally and economically attractive source of energy. To fulfill this purpose, we have adopted three primary goals:

- To ensure a smooth, timely transition from scientific research on fusion to engineering development and practical application.
- To foster cooperation in fusion research and development among all public and private organizations including government, universities, national laboratories and industry.
- To establish increased public awareness and understanding of the potential of fusion energy.

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Dr. DEAN. We tried to boil our policy recommendations down to as few statements as we possible could, and we came up with three policy recommendations which I would like to read.

The first is that we believe "the expeditious development of practical fusion power should receive high national and international priority." We believe that the problem right now is that the program does not have a sufficient priority in the administration's thinking to warrant getting on with the job, and the administration's policy is not oriented towards practical fusion power. It's oriented towards some modest priority research program. And so our first policy recommendation is to get the priority of the program raised and to orient it towards the development of practical fusion power.

Our second policy recommendation is that "the engineering aspects of fusion development require substantial additional effort now." For several years various committees and panel chairmen have come before the Department and this committee saying that the program is ready for engineering development, and if we don't enter into engineering development, we will never have a practical fusion reactor. We will have a lot more knowledge, a lot more science, but we will never have a practical fusion product, no matter how much money we spend, unless we start doing the engineering. The program is ready to do the engineering and we need to embark on that engineering. A lot of that engineering should be done in our industry. If we want to have a practical fusion product, if we want to compete with the Japanese, we have got to develop the corresponding industries in this country similar to Hitachi which is building all of the fusion devices in Japan. Otherwise, in the year whatever the United States will be buying fusion reactors from Hitachi. That, it seems to me, is an intolerable situation for this country to be in, having spent as much money on this as we have.

Our third policy recommendation is that we need "commitments to new and improved experimental facilities now in order to insure continued momentum into the 1990's." Several people have said here this morning we are ahead in the world effort. I don't know if we're ahead. We're barely ahead if we're ahead. We are surely headed to be behind by the 1990's if we don't commit to some new facilities. At some point you've gotten all you can get out of the facilities that you've built and it takes 6 or 7 years to build new machines. If we want capability in the nineties, we have got to invest in that now.

The reason we are in such a good position with the TFTR is we made those commitments in the seventies. We have made no major new commitments since the middle of the 1970's. I was on a committee that reviewed and recommended TFTR in 1975. This committee helped put the TFTR in the budget in fiscal 1976. We are now talking about the fiscal 1984 budget year, and we're talking about maybe initiating the next device in 1986. This would be 10 years since the last major commitment for a fusion machine. We have learned a lot in that time and we've got to get on with it; otherwise, it's not clear to me what we think we're spending our money on in this program.

A lot of us are motivated to make fusion power, not to just do plasma physics for the rest of our lives. I think the country de-

serves to get a practical product out of this program, and if it does want that, it has got to commit to new facilities.

So we believe that the policies and the strategies that were in the Fusion Act were correct. They focused the country toward a practical product; they recognized, the act did, that machines and facilities were required. That is what the comprehensive program management plan postpones. It postpones the engineering; it postpones the commitments to major facilities until the nineties. This seems to me to be the fundamental discrepancy between the CPMP and the Fusion Act. It's a matter of practical focus, of building facilities, as opposed to doing primarily science and postponing engineering and postponing commitments to major facilities.

So I believe we should stick to the policies of the Fusion Act, even if under present economic circumstances we can't quite meet the dates. I didn't ever personally feel that the dates in the Fusion Act or the budget estimates in the Fusion Act were the essence of the Fusion Act. The essense of the Fusion Act was to focus the program toward developing a practical product and to recognize that we had to build facilities to do that. We would like to start them immediately. But if we can't start them immediately, we will start them a year later or 2 years later, but we have got to start them, and that is the basic strategy of the Fusion Act which is essential. I think we need to hold to that strategy.

Now, in the Fusion Act we called the next step, the fusion engineering device, and we said it was about a billion dollars. A number of studies have been done. Princeton and the Magnetic Fusion Advisory Committee have recommended the TFCX, the Tokamak fusion core experiment. In my opinion, that is effectively a fusion engineering device. It's a billion dollar, next-generation machine. It ignites; it has long equilibrium burn. And for all intents and purposes, if we build that machine, we have met, in my opinion, the primary goal of the Fusion Act, which is to initiate a next generation machine at a cost of about a billion dollars that will advance the science and engineering acomplishments of the program. So I accept the tokamak fusion core experiment, as proposed by Princeton and endorsed by the Magnetic Fusion Advisory Committee, as meeting the requirements of the act to get on with a fusion engineering device.

If we do that expeditiously, Princeton says that that device could be operational perhaps by 1992. That's only a 2-year slippage from the requirements of the act, which says 1990. I think we ought to get on with that. We ought not, as the CPMP does, say that well, we'll do that device in the 1990's after we have made a comparison between tokamaks and mirrors.

To get to a question that you keep raising, Mr. Lujan, I don't believe that moving forward with such a machine in the tokamak concept is a detriment to the alternate concepts. As a matter of fact, I believe it will be, in fact, an incentive to these other concepts. We need to make fusion power. We need to ignite and burn a plasma with whatever magnetic concept is ready to go. These other concepts will come along in their time. They need to be nurtured, and when their physics is ready, they can replace the tokamak geometry if necessary, if they're better. But we will have learned how to handle fusion power and how to make fusion power. Therefore, I

do not believe that one should, for example, make the tokamak wait for mirrors or wait for ancther alternate concept. We need to make fusion power now.

The DOE prepared the CPMP under two restrictions from the Office of Science and Technology Policy, and I blame our present problem on them and not on the OMB, because the OMB is basically taking directions on science and technology policy from the OSTP. The OSTP told the program several years ago—and has maintained this stance—that we will have no major new fusion projects, and we will have flat budgets for the rest of the decade.

Now, if we accept such a policy, and it's maintained, it is sure to produce stagnation and decay in the U.S. fusion program, for reasons which I have just enumerated. The United States, in my opinion, is not the unquestioned leader of world fusion development any more. The fusion programs in the United States the U.S.S.R., Europe, and Japan, have comparable achievements, comparable facilities, comparable momentum, and many people believe, and I tend to believe also, that the Europeans and Japanese have capabilities that they're building which exceed the capabilities which we now have. So I accept the idea that we have comparable leadership with the rest of the world now, and we will loose it in the 1990s if we don't make new commitments.

Our present rate of progress is determined by capital investments and facilities we made in the seventies. This progress will predictably diminish unless we commission new facilities soon. And while international cooperative agreements, in which this administration puts great stock, are important, they have been a substantial factor in fusion progress and they should be encouraged, these agreements, I don't believe, are an effective substitute for focused national efforts and national experimental facilities and test reactors. We must build our own technology, our own infrastructure, develop our own industrial capability, and build our own test reactors, which doesn't mean we can't accept money and people and equipment and joint projects, but it does mean that we can't sit back and expect the rest of the world to move ahead and have us not eventually be the losers, financially and economically, in this country.

So I don't subscribe to the strategy which I interpret from figure 2 of the CPMP, which shows the postponement of fusion reactor engineering activities until the next decade. And I don't believe, if we follow that strategy, with flat budgets, and one machine, an engineering test reactor in the nineties, I don't believe the statement in the CPMP that says this would "result in readiness for private sector decisions on commercial applications in the United States around the turn of the century." I don't know anybody in industry that believes that industry will be able to make commercial decisions on fusion around the turn of the century if we follow the CPMP. If industry isn't involved earlier, if we don't build test facilities in greater numbers than is in the CPMP, and earlier, we will not be ready, the private sector will not be ready, to make commercial decisions around the turn of the century.

I believe that this CPMP deliberately postpones for budgetary reasons the shifting of responsibility to industry for fusion development. It puts this off for at least a decade, and this will make a

corresponding, at best, delay in the achievement of practical fusion power by at least a decade, and we don't have any guarantee that it might not be two or three decades because we're asked to believe that in 1990 the then administration is going to all of a sudden come alive and decide to make these commitments. And if they won't make them now, I don't know that they'll make them then, either. So I think this plan puts off fusion power by at least a decade and probably more.

I think the Office of Fusion Energy has been in a difficult position. I think they have tried, valiantly, I say, and I believe it, to prepare a plan for you for fusion development that would be as responsive as possible to the intent of your Fusion Act, but at the same time subject to the constraints of the Office of Science and Technology Policy that there be no new projects and no funding growth. One director of the U.S. fusion program resigned when he realized that this put him substantially in a position of facing a dilemma. Of course the Department wants to say they're implementing the Fusion Act. But, as a matter of fact, the constraints on the program from OSTP do not implement the Fusion Act, and I don't even believe they implement the intent of the Fusion Act. The key thing that we have to have that's different is we must have commitments to new and improved experimental facilities now in order to get our momentum and our accomplishments going so that we will have progress in the 1990's. And we must have some growth in funding to pay for the facilities, and it is no use to pretend that the intent of the Fusion Act, let alone its goals and schedules, can be followed if the constraints imposed by OSTP are not lifted.

We commend the U.S. Government for making a sustained investment in fusion for 30 years. We especially commend the Congress for its strong endorsement and its attempt to maintain a vigorous fusion effort aimed at practical goals. But we believe the time has come to capitalize on our investments of the last 30 years by initiating now a device in which a fusion plasma would be ignited and undergo a sustained fusion reaction.

Thank you.

[The prepared statement of Dr. Dean follows:]

FUSION POWER ASSOCIATES

2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879

(301) 258-0545

STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
OCTOBER 20, 1983

I appreciate your invitation to testify to this subcommittee on the subject of the Department of Energy's recently-submitted "Comprehensive Program Management Plan" (CPMP) for magnetic fusion energy.

The Board of Directors of Fusion Power Associates has recently issued a new Policy Statement on Fusion Energy Development, which takes into account the mandates of the Magnetic Fusion Energy Engineering Act of 1980 and the failure of the Executive Branch to establish the initiatives called for in the Act.

We urge that fusion energy development be based upon the following recommended policies:

- The expeditious development of practical fusion power should receive high national and international priority.
- The engineering aspects of fusion development require substantial additional effort now.
- Commitments to new and improved experimental facilities are urgently needed to insure continued program momentum and progress into the 1990's.

We reaffirm our conviction that the principles and policies established by law in the Magnetic Fusion Energy Engineering Act of 1980 provide a sound basis for the expeditious development of practical fusion power. Recently the Princeton Plasma Physics Laboratory and the Magnetic Fusion Advisory Committee have recommended the initiation of a major new project called the Tokamak Fusion Core Experiment (TFCX). In my view such a device meets most of the objectives of the Fusion Engineering Device (FED) that the Act requires be operational by 1990. The TFCX, if funded aggressively now, could be operational by 1992, a mere two year slippage from the schedules of the Act. Such a project would provide a near-term, practical focus for fusion engineering development and is a much to be preferred strategy to that portrayed in Figure 7 of the CPMP.

The failure of the Executive Branch to provide increased funding during the past three years or to plan to request increased funding in the near future does not warrant the abandonment of the aggressive, practical goals and milestones of the fusion Act. Rather than adopt the new strategy proposed in the department's Comprehensive Program Management Plan, I would much prefer to shift the target dates for accomplishing the goals of the Act.

The DOE prepared the CPMP under two restrictions placed upon them by the Office of Science and Technology Policy: no major new project starts and no increased budgets for the rest of the decade. Such a policy, if maintained, is sure to produce stagnation and decay in the U.S. fusion program. The U.S. is no longer the unquestioned world leader in fusion development. The fusion programs in the U.S., U.S.S.R., Europe and Japan have comparable accomplishments, facilities and momentum. The present rate of progress in the world program is based on capital investment commitments made in the 1970's. Progress will predictably diminish unless new facilities are commissioned soon. International cooperative agreements have been a substantial factor in fusion progress and should be encouraged. Such agreements, however, are not an effective substitute for focused national efforts, national experimental facilities and test reactors.

I do not subscribe to the proposed strategy (as shown in Figure 2 of the CPMP) of postponing fusion reactor engineering activities until the next decade. Further I do not believe that the plan "could result in readiness for private sector decision on commercial applications in the United States around the turn of the century." (Quote from CPMP.) Significant shifting of responsibility to industry for fusion development is put off in this plan by at least a decade. Our view is that governments should encourage and promote the full participation of industry in the planning, research and development as well as in the engineering and operating aspects of the fusion programs, and that the engineering aspects of fusion development require substantial additional effort now.

The Office of Fusion Energy has tried valiantly to prepare a plan for fusion development that would be as responsive as possible to the intent of the fusion Act while a the same time be subject to the constraints of the OSTP that there be no major new projects and no funding growth. One director of the Office of Fusion resigned when he realized the basic dilemma this poses. Commitments to new and improved experimental facilities are needed now to insure continued program momentum and technical progress in the 1990's. Some growth in funding to pay for these facilities is required. It is no use to pretend that the intent of the fusion Act, let alone that its goals and schedules, can be followed if the constraints imposed by OSTP are not lifted.

We commend the U.S. government for making a sustained investment in fusion research and we especially commend the U.S. Congress for its strong endorsement of maintaining a vigorous fusion effort aimed at practical goals. We believe the time has come to begin to capitalize on this investment by initiating now a device in which a fusion plasma can be ignited and undergo a sustained fusion reaction.

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Mrs. LLOYD. Thank you, Dr. Dean, for your statement and for your very vital role that you played in our fusion program.

You stated that you would prefer to go back to the Fusion Act and with some time slippage rather than adopt a new strategy on the CPMP. What do you think is the most glaring omission in this CPMP?

Dr. DEAN. Well, I think there are a number of problems with the CPMP——

Mrs. LLOYD. Do you think it's basically science for science sake, without looking to the engineering role?

Dr. DEAN. Well, as I mentioned, I think the CPMP makes an attempt to follow the intent of the act, but because of the constraints, that they cannot make any new starts in the eighties, and there is no capability to pay for any facilities by incrementing the budget, even in a minimal way. The CPMP has a dilemma that you can't move the program forward and meet a commercial decision date in the year 2000 by postponing everything in the eighties until the nineties and then just say you're going to do it all in the nineties. It's not credible to the industry to say that we do no new things, no engineering, in the eighties, and in the 1990's we're going to build an engineering test reactor and all these great things will be done in the nineties, and then in the year 2000 industry will make commercial decisions. It is not technically credible. Industry needs to be involved in the eighties in a substantial way. We need to embark on power-producing facilities now and another generation of power-producing facilities in the nineties, in order for that potential to exist.

Mrs. LLOYD. What kind of budget do you really think that we should have for the 1980's to really capitalize on our program opportunities?

Dr. DEAN. I believe that the operating budget of the program has to have modest growth, at least cost-of-living, and preferably a little bit more. And then, on top of that, you must, as I mentioned, pay the cost of whatever facility you approve as a line item.

I don't know what the cost of that facility is until a proposal is made, but it's been estimated to be between a half-a-billion and a little over a billion of construction cost. Those costs will have to be spread out over a number of years to pay for the facility. If it runs—if it starts in 1986 and runs to 1992, you've got a profile of costs and the largest cost, the major cost will come around 1989, 1990, 1991, not in the near term.

I can't predict for sure what the ERAB Panel will say. You know, the Fusion Act requires the ERAB to review the fusion program every 3 years, and they are reviewing it right now. They are supposed to report next month. I was at a hearing that they held, a public hearing in Oak Ridge last month, and there seemed to be a consensus in that committee that the embarcation on the TFCX was the highest priority in the fusion program and I believe they will tell you—I can't say this for sure—that you should embark on this even under flat budgets, but recognize that come later in the decade you will have some incremental funding required to pay for the increased costs. But if we wait until the administration guarantees that they're going to put up the money up front, I don't think we'll ever get that commitment. We have got to make the commit-

ment in the present program to embark on that machine. I think that's what they'll say, and then fight for the money that's required to complete the construction.

So I don't know what the exact dollar level is. I accept the MFAC statement that somewhere between 25 and 40 percent increase in the budget will eventually be required. But the way I look at it, it's not so much a new level of the program; it's somewhat like the old level, but then you pay on top for the costs of that billion dollar construction project, and maybe you can get it for a little less.

Mrs. LLOYD. What do you think industry's role should be in the TFCX and MFTF-B upgrade?

Dr. DEAN. I believe that these projects should be—their goals and objectives should be defined by the laboratories and that these projects should be put out on a competitive bid for engineering design and construction as projects.

Mrs. LLOYD. Engineering design primarily?

Dr. DEAN. I believe that the responsibility for engineering design and construction, assembly and so on, should be put out to industry on a competitive bid basis, but that the overall design and objectives, conceptual design and objectives, should be determined by the Princeton Plasma Physics Laboratory as being the most knowledgeable tokamak group and Lawrence Livermore Laboratory for the mirrow device..

Mrs. LLOYD. How about the operational, the——

Dr. DEAN. I believe that industry should be brought into the operations, not only of the next generation machine, but of the present generation of machines. There are many jobs on these machines that industry could be contracted with to do. And as an example, Livermore has brought in TRW, as you probably know, in a substantial way now to complete the MFTF with the intent that industry will also be involved in its operation, and I believe that Princeton has done a number of similar things now, of planning to bring industry into the laboratory. I commend that process.

Mrs. LLOYD. Would you briefly just comment on the Japanese and the European approaches to program planning versus ours?

Dr. DEAN. Maybe I take too much pride in this, but it was my perception in 1976, when we were really going after planning, that the Europeans and Japanese were not planning at all. And after we published our plan, they went through it with a fine-tooth comb and within a year or so they had plans which were almost identical. Now we have changed our direction several times and rewritten our plan several times, but they have stuck pretty much to their original plan. I think most of us now feel that the Europeans and the Japanese will articulate a better plan than we articulate, with more of a commitment to schedules and so on.

Now, personally, I don't believe that they will actually embark on their next-generation machines all by themselves if we lag behind. I don't think they're quite confident enough to do that yet. But with the accomplishments that they will, particularly in Europe, get out of JET, and the experience they're getting with ASDEX and super-conducting machines in France, I believe the chances are that pretty soon they may feel confident enough to go it alone.

Certainly in Europe, I think they have identified the site of the next generation machine to be at Cardavesh. They will not formally admit to that, but it's quite clear to me that the whole French program is moving to Cardavesh with the intent of having that be the future site of a fusion power reactor.

The Japanese have the site already picked for their experimental power reactor in fusion. They've got the land right there next to the JT-60 and on their charts they have it labeled "site of the experimental power reactor."

We on the other hand have no idea where we're going to put our power reactor, none. We have never faced up to the question of site for the development of fusion. This committee tried to get us to do that in the Fusion Act by calling for a center for fusion engineering, and that has just been completely ignored by the administration.

Mrs. LLOYD. Thank you very much, Dr. Dean. I appreciate your testimony.

Again, we will be submitting questions in writing for all of our fine witnesses that we would like to put in the record as well. Thank you.

If there is no further business, the committee stands adjourned.

[Whereupon, at 11:20 a.m., the subcommittee was adjourned.]

APPENDIX

Department of Energy
Washington, D.C. 20585

December 6, 1983

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy Research
and Production
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Mrs. Lloyd:

On October 20, 1983, John F. Clarke and Ronald C. Davidson appeared before the Science and Technology Subcommittee on Energy Research and Production concerning the Comprehensive Program Management Plan for magnetic fusion energy. On page 27, line 620, of the transcript, you stated the record would be open for additional questions. Enclosed are answers to questions you sent Dr. Clarke in your letter dated November 1, 1983.

If you have any questions, please call Ingrid Nelson or Tom Pretorius of my staff on 252-4277. They will be happy to assist you.

Sincerely,

for Thomas H. Pretorius
Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

cc: Ms. Nancy Jeffery
Subcommittee Staff

POST-HEARING QUESTIONS AND ANSWERS

Relating to

OCTOBER 20, 1983 HEARING

Before the

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

COMMITTEE ON SCIENCE AND TECHNOLOGY

U.S. HOUSE OF REPRESENTATIVES

WITNESS: JOHN F. CLARKE

ASSOCIATE DIRECTOR, OFFICE OF FUSION ENERGY

OFFICE OF ENERGY RESEARCH

DEPARTMENT OF ENERGY

. 1. For the record, please furnish a list of the additional activities which would be undertaken, if the total program funding was increased by first 5 percent and secondly by 15 percent in FY1985 from FY1984 levels. Would you also generally characterize what might be done additionally at these two enhanced levels?

. The base program currently supported by this Administration is an adequate one to meet the objectives of the Comprehensive Program Management Plan and I can assure you that we are addressing the critical issues in the program. Let me address your question by pointing out what the "Fusion Community" believes to be the highest priority items on the margin:

First, priority within the existing base program is in the following specific areas: strengthening the preparations for Tokamak Fusion Test Reactor operation with deuterium-tritium, improving selected programs on critical technology issues, and providing additional flexibility to the supporting concepts research program.

Next comes the capabilities to accomodate future initiatives in the fusion program such as the Tokamak Fusion Core Experiment. As indicated in my testimony, this experiment would be used to investigate ignition, long pulse equilibrium burn, and the associated technologies.

These areas of research are consistent with those areas discussed by our Magnetic Fusion Advisory Committee and at the recent Energy Research Advisory Board hearing on Magnetic Fusion.

Q. 2. Please provide a breakdown of where the 30 percent of the Office of Fusion Energy budget, dedicated to industry, is being spent.

A. The following list reflects the major industrial participants in the fusion program including both direct contracts from DOE and subcontracts with the national laboratories.

In addition to the major participants, there are many thousands of companies providing less than $100,000 annually in goods and services to the Office of Fusion Energy.

OFFICE OF FUSION ENERGY
INDUSTRIAL CONTRACTS
FY 1982
(Dollars in Thousands)

Electro-Engineering	100K-200K
Tymnet	
Grinnel Systems	
Electronic Enclosures	
Devault of Delaware	
Martell Construction Company	
Peifer	
Raytheon	
Ingersoll-Rand	
Balzers	
Ken Weld	
Foster Engineering	
Jersey Precast	
Validyne Engineering	
Advanced Environment	
Magnetic Metals Company	
Hitron Corporation	
Alpha Industries	
Fiberguide Ind.	
Gulton Ind.	
Henley and Company	
Everson Electric Company	
Xerox	
Applied Microwave Plasma Concepts	200K-400K
Rotator	
Amuneal Manufacturing	
Energy Research Associates	
American Ind. Tech.	
Space Company	
Joerger Enterprises	
Austin Research Associates	
Mid-Atlantic	
Pacific Gas and Electrical (PG&E)	
S. T. Peterson	
Ford Construction	
Eastern High Voltage	
American Science and Engineering	
Cornell and Underhill	
Rockwell	
Grand Cal	400K-1,000K
American Satellite	
Control Data Corporation	
Southern Bolt	
Airco Industrial Gases	
Air Preheater	
Brown Boveri	
Jaycor	
Cray Research	
Grumman Aerospace	
Mathematical Sciences, NW	

Giffels Associates, Inc.	1,000K-2,000K
Gould	
Transrex	
Universal Voltro	
Combustion Engineering	
Airco, Inc.	
Perkin-Elmer	
Science Applications	
Hughes Aircraft Company	
CVI Corporation	
Allis Chalmers	
RCA	2,000K-5,000K
Carl N. Swenson	
General Electric	
Avdin, Inc.	
Varian Associates	
Digital Equipment Company	
TRW, Inc.	
Ebasco/Grumman	
Chicage Bridge	
Pittsburgh - Des Moines	
Westinghouse	
Terminal Construction Company	
None	5,000K - 10,000K
McDonnell-Douglas Astronautics Company	> 10,000K
General Dynamics Corporation, Convair Division	
GA Technologies, Inc.	

Q. 3. Dr. Dean, in his testimony, pointed out that the U.S. stands to lose its preeminence in fusion development because there is little opportunity for including line-item construction work in the budgets. He stressed the point that progress is dependent on moving ahead with such projects. If progress does depend on moving forward with the next major project, what experiments and activities presently funded can be cut out so that some of the "flat funded" operating money can be reprogrammed into a line-item project such as TFCX?

A. The present fusion program aims at maintaining a sufficient effort on all of the relevant research topics to ensure that practical application is not precluded by overlooking some critical ingredient. This is what we mean by maintaining a balanced program. We believe it would be extremely risky to upset this balance. However, by the late 1980s, much of the work in each critical area can be redirected to support a TFCX project since that project is itself extremely relevant to issues of ultimate application. In this way, we believe that as much as half the effort for a fusion core experiment can be provided from the current program. Much of this support could be provided by the early and rapid shutdown of the TFTR program as soon as its principal objectives are met. This action would provide both funds and personnel to support design, development, and project management needs. We could also redirect a substantial part of the development and technology program now addressing critical generic technology issues to address similar, but project-specific development requirements. Undertaking such a project would also require the close coordination of every relevant experimental and theoretical effort within the present tokamak program with the project's technical requirements. These two actions would directly reduce any new incremental PACE and operating funds needed above the current fusion program resource level.

Q. 4. What is your opinion of the probability of producing electricity commercially from D-T (Deuterium-Tritium) fuels in a fusion reactor?

A. I believe the probability is high that D-T fusion reactors could be used to produce electricity commercially in the next century.

We have performed studies of commercial fusion reactors over the last several years using knowledgeable groups of industrial, laboratory and university professionals. These studies have shown that fusion plants will be comparable to fission plants in size and cost. Furthermore, both the tokamak and tandem mirror machine configurations have shown themselves to be practical for assembly/disassembly and maintenance.

Q. 5. Given the vast complexities of the fusion program, what management tools, such as PERT techniques (Program Evaluation Review Technique), have you employed to optimize the integration of activities toward specific objectives?

A. The fusion program is managed using a variety of techniques as appropriate for each task. The projects, for example, are managed very effectively with PERT-like techniques employed in great detail within a complete Performance Measurement System. The Confinement Systems operating programs use annually negotiated milestones in a Management by Objective process. Each of the subprograms, development and technology, applied plasma physics, toroidal confinement systems, and mirror confinement systems, has an overall program plan that is updated every two or three years as new information warrants it. These program plans cover technical objectives, status, R&D needs and intended contributions from the various facilities. These plans are developed through intensive discussions and iterations by knowledgable representatives of each technical community. Each of these plans is reviewed by both the OFE managers and specially constituted review groups containing experts from within and outside of the particular community involved. In addition to peer review, which makes up the largest percentage of the total number of reviews, we use the assistance of the Secretary's Magnetic Fusion Advisory Committee (MFAC) and Energy Research Advisory Board (ERAB) and the National Academy of Sciences/National Research Council (NAS/NRC). With these aids, the senior management of OFE reviews every element of the program budget for quality, necessity, and relevance to our principal goals.

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Q. 6. Given the lower core densities in fusion, compared to fission, what is your opinion of the probable size of a commercial fusion reactor compared to a fission reactor of similar power, and what do you suppose would be the relative cost of each?

A. Both the Starfire (Tokamak) and MARS (Tandem Mirror) designs for a commercial power fusion reactor are for about 1200 MW of net electric power, a value comparable to present large fission reactors. The physical size of the nuclear part of a fusion plant is nearly twice that of the nuclear part of a fission plant, but overall power plant size is similar. The direct capital cost as estimated by the Starfire design ($1700/KWe) and the MARS design ($2000/KWe) is somewhat higher than that of some light-water fission reactors. However, the cost of fuel and the operating cost for a fusion plant is expected to be less than that for a fission plant. As a result, the above studies indicate a similar cost for fusion and light water reactor electricity. While there are many uncertainties in predicting the cost of energy for future fusion reactors, these studies give no fundamental reason to believe that the cost of electricity from fusion plants cannot be economically competitive.

The fusion program continues to work toward attractive reactor concepts. Maintainable machine configurations, use of low activation materials, improvements in understanding plasma confinement, thermal barriers in the tandem mirror and steady state operation of the tokamak are recent developments directed at reducing costs and complexity of a reactor. Studies directed toward cost-effective approaches to fusion power will continue.

Q. 7. What has been learned about tritium handling and what problems, if any, are envisioned with regard to D-T experiments at Princeton?

A. The United States has been safely handling and controlling different quantities and forms of tritium for many years, primarily in its defense programs. It is upon the unclassified portion of this existing information base that the Fusion Energy Program has been developing its tritium processing and handling technology. A major part of our goal to demonstrate the ability to control, contain, and handle reactor relevant quantities of tritium will be accomplished with the operation of the Tritium Systems Test Assembly (TSTA) at Los Alamos National Laboratory. The objective of TSTA, which will begin tritium operation early in 1984, is to develop, demonstrate, and interface the technologies related to the deuterium-tritium fuel cycle for fusion reactor systems. TSTA, which utilizes 150 grams of tritium, has the capability of processing up to 1 kilogram of tritium per day through its closed loop fuel cycle system. By comparison, the Tokamak Fusion Test Reactor (TFTR) at Princeton will have only 5 grams of tritium on site. Thus, this combination of past experiences in other DOE programs, along with the operation of TSTA, will form a significant base of experience upon which the program expects to conduct successful D-T experiments in the TFTR facility at Princeton.

Q. 8. Please furnish for the record a general description of the management methods used by the Department of Energy, Office of Fusion Energy, including such items as procurement practices, program scheduling, status reporting and review and other elements of the reporting, management and control process?

A. In response to this question, I would like to review briefly how individual tasks and projects are managed within the program.

Individual tasks and projects are selected by OFE for inclusion in the program plans based on specific proposals developed by fusion laboratories and operating contractors or unsolicited proposals followed by expert peer review by the fusion community. Projects are evaluated by OFE on their technical merit in advancing the U.S. and world fusion program, and on their technical feasibility, adequacy of planning, overall cost and schedule credibility, and relative priority considering annual program budget limitations. Within the current fusion budget, new initiatives are subjected to very close scrutiny to assure feasibility and high payoff potential.

OFE is the DOE Headquarters program office involved primarily in program formulation and program oversight, whereas all program execution is accomplished in the field by national laboratories, universities or industrial contractors working under contracts administered by DOE field offices. OFE manages the execution of the program plans with the support of field offices. Milestones are established for the toroidal and mirror programs and individual development and technology program tasks. Selected major milestones are designated as OFE controlled, while the control of other supporting milestones are field office or contractor controlled. Control is based on information received from contractors and field offices in reports and through frequent onsite

reviews. Because of the close working relationship between the OFE professionals responsible for R&D programs and their field office and contractor counterparts, frequent and routine telephone communication is maintained to discuss progress and problems. OFE program managers report periodically on program progress to OFE management.

The management systems specified in DOE Project Management and Major System Acquisition Orders are used to manage OFE projects. The principal elements are as follows:

1. Summary Project Plans on Major System Acquisitions, such as TFTR and MFTF-B, are approved at Headquarters by OFE, the Director of Energy Research, and the acquisition executive, currently the Assistant Secretary for Management and Administration. These plans define overall project objectives, scope, cost, schedule and funding parameters.

2. Detailed Project Management Plans are developed by the DOE field office and operating contractor and approved by OFE. These plans define responsibilities; project organizational arrangements; cost, schedule and technical baselines; and the management procedures for controlling cost, schedule and technical baselines. The project management plans are updated periodically to ensure accuracy.

3. Monthly reporting by the contractors and field offices in a prescribed format which tracks cost, schedule and technical performance.

4. Monthly project management meetings between the DOE field office and contractor to track progress and problem resolution.

5. Quarterly and semiannual project reviews with OFE Headquarters participation wherein detailed evaluations are conducted of cost and schedule baselines, actual progress and forecast performance.

6. Total project cost and overall schedule milestone control by OFE with project estimate contingencies controlled by the DOE field office.

In addition to these overall management arrangements, a disciplined process is maintained over the technical design of the project via the approval of overall and individual system technical requirements followed by preliminary and final design reviews. Independent technical review is stressed at the preliminary and final design review stages of each system design to assure feasibility, economy and compliance with technical requirements. Configuration control of the design is maintained during the design, fabrication and installation process via agreed upon change control procedures. Quality assurance is maintained via agreed upon Q/A policies and procedures and period audits against their procedures.

It should be noted that in addition to applying the above procedures to large fusion project such as TFTR and MFTF-B, OFE also utilizes these same management concepts on smaller project tasks such as ATF-I, TMX-U, LCTF, D-III Upgrade and Vessel Modification, and TARA.

Procurement management in fusion complies with the Federal and DOE procurement regulations. The primary responsibility for assuring this compliance resides with the DOE field offices who award and administer the contracts with fusion laboratories, operating contractors, and off-site university and industrial contractors. They ensure that competitive procurements are used to the maximum extent possible and that noncompetitive procurements are well justified in accordance with applicable regulations. OFE performs the same functions in its contracting. The field offices also review the procurement management systems employed by major fusion contractors to assure they are in compliance with Federal laws and regulations and they review individual subcontract procurement actions above certain dollars thresholds or subcontracts of special types or unusual nature.

The program emphasizes advanced procurement planning to assure early establishment of business strategies which will ensure that project and cost schedules are met. In developing these strategies both Headquarters and field technical personnel as well as the expertise of the procurement and legal staffs are used to ensure sound planning.

Overall, we consider the management practices utilized in fusion to be well disciplined.

Q. 9. Do you believe your Program activity, in consideration of meeting objectives as soon and as effectively as possible, is properly distributed among industry, the National Laboratories and universities?

A. The distribution of program activities among industries, national laboratories and universities is determined on various grounds including historical development, current capabilities, and broader national domestic policy. We believe the current distribution of activities is about right to achieve the program's objectives as soon and as effectively as possible. Our principal concern is to ensure that within this distribution the quality of participation is as high as possible.

With regard to industrial participation, there is both a significant amount of the total program, more than one third, spent in industry and a broad range of kinds of involvement in the program. Our objectives in recent years have been to strengthen the industrial involvement in the planning and implementation aspects of the program while retaining the traditional involvement in supplying equipment and services. In addition to the well-known, large-scale involvement of GA Technologies, Inc. in building and operating the Doublet III tokamak and by Ebasco Services, Inc. and Grumman Aerospace Co. in designing, developing, constructing, assembling and testing TFTR, we have initiated other advances in both project and program areas. One new approach has been the employment of a group of competitively selected industrial teams working in the Fusion Energy Design Center (FEDC) at ORNL. These industrial personnel are supporting the maturation of the advanced system studies design process and have earned

a high degree of respect throughout the technical community. Most recently, a combined industry-utilities team won a competition to provide technical support services to OFE in developing standards and techniques for evaluating reactor systems studies and modifying the supporting reactor and development program. This kind of activity is in our view improving the quality of participation. This concept of encouraging industrial participation in selected areas of greatest current impact is supported by the European Community's (EC's) Beckurts Panel and the NAS/NRC panel on future engineering needs. To ensure that we have looked broadly at the question of industry's role in fusion, we have asked the MFAC to address this specific issue in its current deliberations. MFAC was set up with and continues to include at least one-fourth of its members from industry.

The national laboratories have carried and are continuing to carry the principal technical burden in the fusion program. To prepare for the transition to industry, we encourage, and the laboratories support, broader participation in each of our individual nation-wide subprograms addressing a particular technical problem. This emphasizes widespread communication among all university, laboratory and industry participants rather than a parochial, institutional isolation. The laboratories are also investigating more operational support of large projects by industry.

Q. 10. John M. Dawson, UCLA, noted in his outline of UCLA's Plasma and Fusion Program that "DOE tends to concentrate its attention on the efforts at national labs..."; "that much of the good university research gets overlooked..."; and "Many innovative ideas that could be of great value to the program are never funded because they are not of immediate applicability to one of the major devices." The CPMP identifies TFTR (Tokamak Fusion Test Reactor) and MFTF (Mirror Fusion Test Facility) operation as objectives. Would you comment on Dr. Dawson's statement in the context of the CPMP?

A. The Office of Fusion Energy has always welcomed the presentation of new ideas for advancing the program. Based on thorough review and evaluation of the many unsolicited proposals received by the Department of Energy, I believe that very little university research that is relevant to advancement of the fusion program's objectives is overlooked. Immediate applicability to one of the major devices is not a general criterion used to evaluate university research proposals. The fusion program has many development objectives, ranging from those requiring the operation of TFTR and MFTF-B to improved confinement concepts to specific technical issues as described in the CPMP, to which university research is expected to contribute significantly.

Q. 11. Should additional research be conducted on understanding the causes of experimental thermal conductivity losses from tokamaks which exceed theoretical predictions?

A. I do not think we need additional research on anomalous thermal conductivity—not because understanding anomalous thermal conductivity is complete or is not important, but because a substantial amount of research is being done on this topic. Research on thermal conductivity or energy confinement is a research activity on all tokamaks and a major one on the TEXT device at the University of Texas.

Q. 12. Are additional small to medium size facilities, such as those at MIT and the University of Texas, needed for experimental and theoretical work? Does the CPMP strategy provide for them?

A. No new devices of this type are currently planned because we believe that those present facilities are adequate to contribute to the technical base needs of the program. The CPMP does allow for new facilities if they are identified and determined to be of high priority.

Q. 13. What is the purpose of the ATF-1 (Advanced Toroidal Facility) experiment; what results do you anticipate; and how does this work relate to the overall Magnetic Fusion Program?

A. ATF-1 is designed to operate in the so-called "second stability regime" of toroidal machines which is a regime of considerable interest for tokamak-type reactors. Also, the physics of heat transport in the collisionless regime in the helical magnetic ripple is closely related to the problem of heat transport in tokamaks with the rippled magnetic fields which arise out of the finite number of toroidal coils. ATF-1 is an experiment which has been designed to make major advances in the stellarator line of toroidal research as well as enhance the toroidal data base in areas which are particularly relevant to optimizing the tokamak as a reactor.

Q. 14. On March 23, 1983, Dr. Trivelpiece forwarded a document to the Subcommittee entitled "FY 1984 Program Summary Document, Magnetic Fusion Energy" dated January 27, 1983. In that document it is mentioned that work was to have begun on the ETR this month. What is DOE's intentions regarding the ETR?

A. DOE is beginning conceptual design of a tandem mirror ETR and a tokamak ETR in FY 1984. The on-going INTOR critical issues work will form an important part of the tokamak ETR study. Total effort in FY84 and 85 is estimated at 60 person-years. The intention of the DOE program is to maintain up-to-date ETR designs for the tokamak and tandem mirror concept throughout the decade. These designs will provide a basis for comparison of the attractiveness of the reactor concepts and will help guide the ongoing fusion R&D program.

Q. 15. In the 1984 Program Summary for Magnetic Fusion Energy (January 27, 1983) one view-graph stated, "We are now at a critical decade for industrial involvement." What industrial participation is contemplated in FY 1984 and do you plan for increasing industrial participation in the 80's?

A. Our plans for industrial participation in the 1980's depend upon any potential new missions and new ideas on participation. With regard to the last item, MFAC has been asked for its advice on appropriate roles for industrial participation. We expect a report by June 1984. With regard to preparation of design studies for any future decision on a major new facility, we are planning to invite industrial competition for design and engineering services supporting the involved laboratory groups. Finally, since over one-third of the budget is now spent in industry, any near-term increase in industrial participation would likely come in terms of quality, not quantity.

Q. 16. In your written testimony, you mentioned the recent excellent results from the TFTR program at Princeton. Will you elaborate and tell us more about these successes?

A. During the first series of experiments, TFTR was operated at toroidal fields up to 2.7 Tesla (about one-half of its ultimate capability) and currents up to 1 megampere (slightly more than one-third of its ultimate capability). Both hydrogen and deuterium discharges were run at densities up to $2 \times 10^{13} cm^{-3}$. For these experiments, the major radius was 250 cm, and the minor radius was limited to 68 cm with carbon rail limiters. Preliminary results from the confinement time analyses indicate an energy confinement time of 150-180 msec for plasmas with an electron density of $2 \times 10^{13} cm^{-3}$ and electron temperatures of 1.5 keV. This confinement time exceeds that expected from INTOR scaling by about a factor of three. This result is important because it suggests that the energy confinement increases as the cube of the plasma size, rather than the square of the plasma size as INTOR scaling predicts. This result confirms that TFTR will be able to achieve its goal of demonstrating energy breakeven.

Q. 17. Will you furnish for the record a program breakdown of the principal work tasks together with the organization responsible for each task, the schedule and cost estimate for each task, and the primary interrelationships between tasks for the fusion base scientific and engineering program and the major projects?

A. The fusion program is carried out in a large number of interrelated tasks by a wide variety of institutional and individual participants using a broad range of facilities. A program breakdown of the principal work tasks can be developed with the aid of the accompanying figures.

Figures 1 and 2 illustrate the overall program strategy emphasizing first the three successive phases of the program and second the consent of a broad base program that can be complemented, where appropriate, by a reactor engineering program. The current fusion program fits entirely with the "base program" including only the planning function for the reactor engineering program.

This program is carried out within the Office of Fusion Energy by five Divisions whose functions reflect the basic tasks and interrelationships within the program. The Toroidal Confinement Systems Division and the Mirror Confinement Systems Division direct the major experimental efforts to produce and control fusion plasma. The Development and Technology Division directs efforts to provide technological support for current confinement experiments and develop critical elements of advanced technology for next steps in the program. Similarly, the Applied Plasma Physics Division directs research programs supportive of current major experiments and develops new theoretical, analytical, computational and diagnostic tools for

advancing fusion systems. This Division also directs work on new, qualitatively improved ideas for confinement concept improvement and it manages the program's computer support system. Clearly, these four Divisional programs are closely integrated. The Confinement Systems Division generates technical requirements for improved technology, theory, etc. while providing new data on plasma behavior and operational experience. The D&T and APP Divisions provide improved technical bases for the experiments and suggest possible new or improved avenues of design and implementation. The fifth Division, Planning and Projects, works closely with the other four Divisions providing project management of major new facilities during construction and also provides program-wide planning and international cooperation, coordination, and management.

Figure 3 illustrates the basic ordering of activities in the program. The pulsed tokamak program is directed toward scientific feasibility and because of its advanced status carries with it much of the basis for the successive technology and engineering feasibility work. The long pulse tokamak, which is the current advanced stage of tokamak research, and the tandem mirror programs represent the two principal reactor concept options we expect to have considerable information on by the end of this decade. The other concepts that represent potentially qualitative improvements or ways to new insights are pursued at a scientifically driven rather than programmatically driven pace. The technology and applied plasma physics programs reflect these priorities as well.

The principal confinement facilities supporting this ordering are shown in Figure 4. TFTR, with its supporting experiments, is aimed at scientific feasibility demonstration and understanding; MFTF-B, with its supporting experiments, is aimed at establishing the tandem mirror reactor-level plasma knowledge base; D-III Upgrade is pursuing the extension to longer pulse lengths of the physics of reactor-level hydrogen tokamak plasmas.

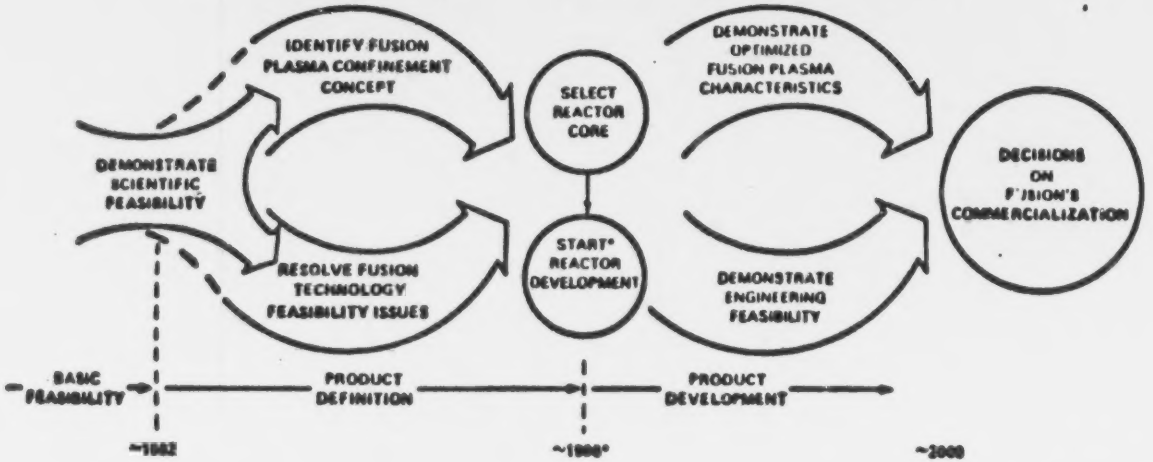
Figure 5 illustrates the current annual budgets for the six major participating fusion centers. This listing can be translated approximately into the costs of the three principal facility programs. To a large extent, the budget at PPPL represents the funds required for the TFTR program and supporting experiments including PLT/PDX. Similarly, the LLNL budget represents funds required for the MFTF-B program and its supporting experiment TMX-U. In the same way, the GA Technologies, Inc. budget is principally directed toward the D-III facility program. The TFTR program is currently committed through 1986 and plans are now being developed and reviewed for the following few years. The MFTF-B program is committed through the end of the decade and further plans are being developed. The D-III facility program is committed through 1988.

The interrelationships among tasks can best be understood by reference to Figures 6 and 7; these figures indicate the facts that the physics issues must be addressed as a system whereas the technology topics can at first be addressed independently. The role of the ETR, shown

on Figure 4, can now be understood as the necessary systems integration test.

A more detailed discussion of the implementation of these investigations may be found in the charts contained in the FY 1984 Program Summary Document for Magnetic Fusion Energy. These charts illustrate the objectives and strategy, facilities and tasks, and participants and budgets for each sub-program.

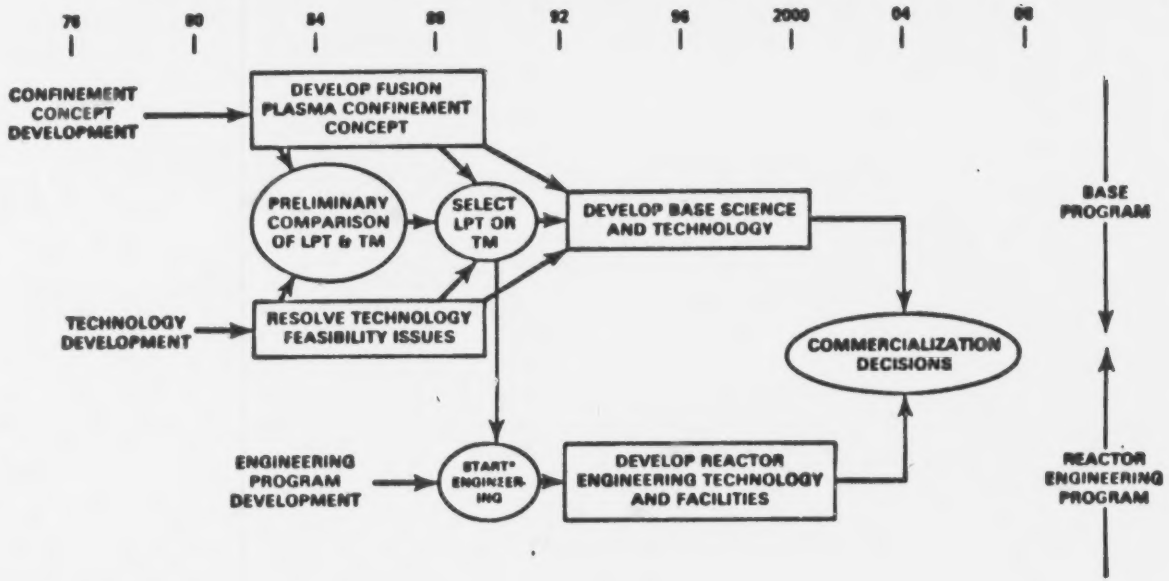
THE REVISED FUSION PROGRAM STRATEGY SUPPORTS THE PRESIDENT'S VIEW OF A FEDERAL FUSION ROLE IN A TECHNICALLY AND FISCALLY SOUND MANNER



- RESOLVE BASIC SCIENTIFIC ISSUES
- IDENTIFY TECHNOLOGY ISSUES
- CONDUCT APPLIED SCIENTIFIC RESEARCH
- DEVELOP TECHNOLOGY BASE
- OPTIMIZE FUSION REACTOR PLASMA
- DEMONSTRATE REACTOR TECHNOLOGY

* TIMING OF START DEPENDS ON TECHNICAL PROGRESS AND EXTERNAL PERCEPTION OF NEED.

THE FUSION PROGRAM STRATEGY CONSISTS OF A BASE PROGRAM AND A REACTOR ENGINEERING PROGRAM PURSUED IN THREE SEQUENTIAL PHASES



PROGRAM PHASES AND TASKS			
	• SCIENTIFIC FEASIBILITY	• PRODUCT DEFINITION	• PRODUCT DEVELOPMENT
	• RESOLVE BASIC SCIENTIFIC ISSUES	• CONDUCT APPLIED SCIENTIFIC RESEARCH	• OPTIMIZE FUSION REACTOR PLASMA
	• IDENTIFY TECHNOLOGY ISSUES	• DEVELOP TECHNOLOGY BASE	• DEMONSTRATE REACTOR TECHNOLOGY

*TIMING OF START DEPENDS ON TECHNICAL PROGRESS AND EXTERNAL PERCEPTION OF NEED.

CURRENT TECHNICAL STATUS AND OUTLOOK OVER THIS NEXT DECADE YIELD CLEAR ORDERING AND ROLES FOR ALL CONCEPT LINES

Role	Ordering		Timing of Reactor Candidacy Readiness	Priority & Funding
Existing Base* for Scientific and Engineering Feasibility	Pulsed Tokamak		Early '80's	1
Developing** Reactor Options	Long Pulse Tokamak	Tandem Mirror	Mid-to-Late '80's	2
Stimulation*** of New Ideas and Possible Reactor Improvements	Bumpy Torus, Stellarator, RFP, Compact Toroids		—	3

MFAC —

Principal Findings and Recommendations

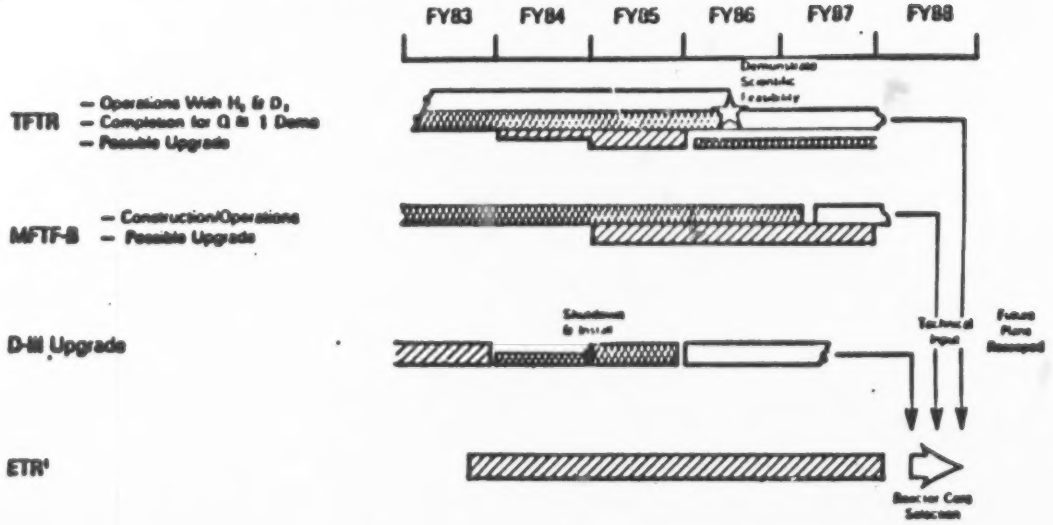
* "In the case of the tokamak, the existing data base implies the feasibility of net power production."

** "At present, the tandem mir[illegible] tokamak concepts can be embodied in viable reactor designs of ro[illegible]ilar characteristics. ...both designs also have the potential for signific[illegible] further improvement through technical innovations that are in the exploratory phase."

*** "Program emphasis should be placed on the development of backup concepts that show significant promise of an improved reactor product as well as a strong scientific data base."

The Existing Major Experimental Facilities And The ETR Are The Central Elements Of The Implementation Process

Construction/Operation Sequence



/Planning will include reference designs to focus & guide R&D.

THE MAJORITY OF THE PROGRAM IS CONCENTRATED IN SIX MAJOR FUSION R&D CENTERS

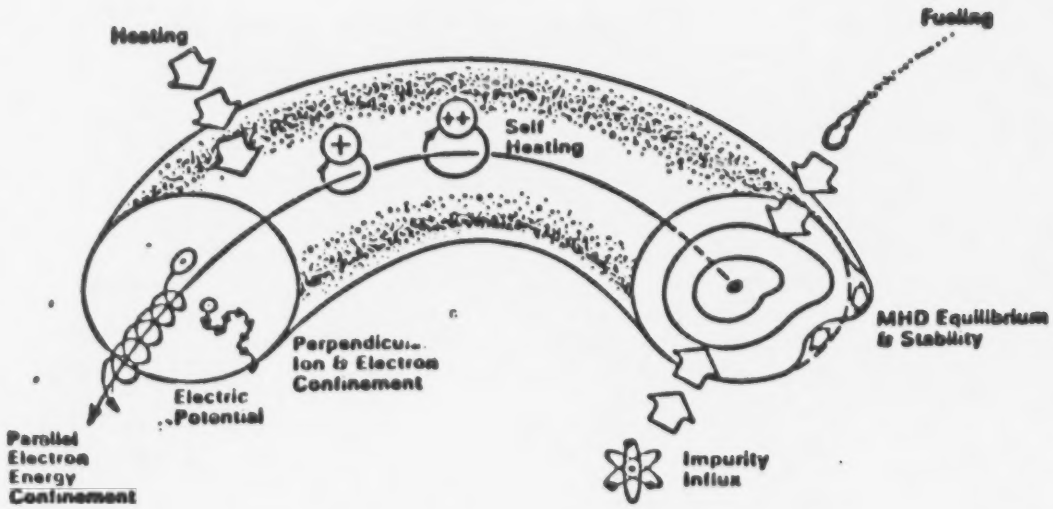
Major Contractors	Emphasis	Total FY84 Budget (BA $ In Millions)	Percent
Princeton	Tokamak	130.8	28
Oak Ridge	Tokamak, EBT, Technology	71.3	15
Lawrence Livermore	Mirror	106.8	23
GA Technologies, Inc.	Tokamak	38.5	8
Los Alamos	Alternate Concepts	19.7	4
MIT	Tokamak, Mirror	24.1	5
Others*		55.1	12
Universities		24.4	5
		470.7	100

* Includes ANL, BNL, LBL, HEDL, PNL, INEL, SANDIA, Direct Industrial Contracts (excluding GA) and Program Direction

OUR PROGRAM MANAGEMENT IS
BASED UPON FUSION'S FUNDAMENTAL
CHARACTERISTICS

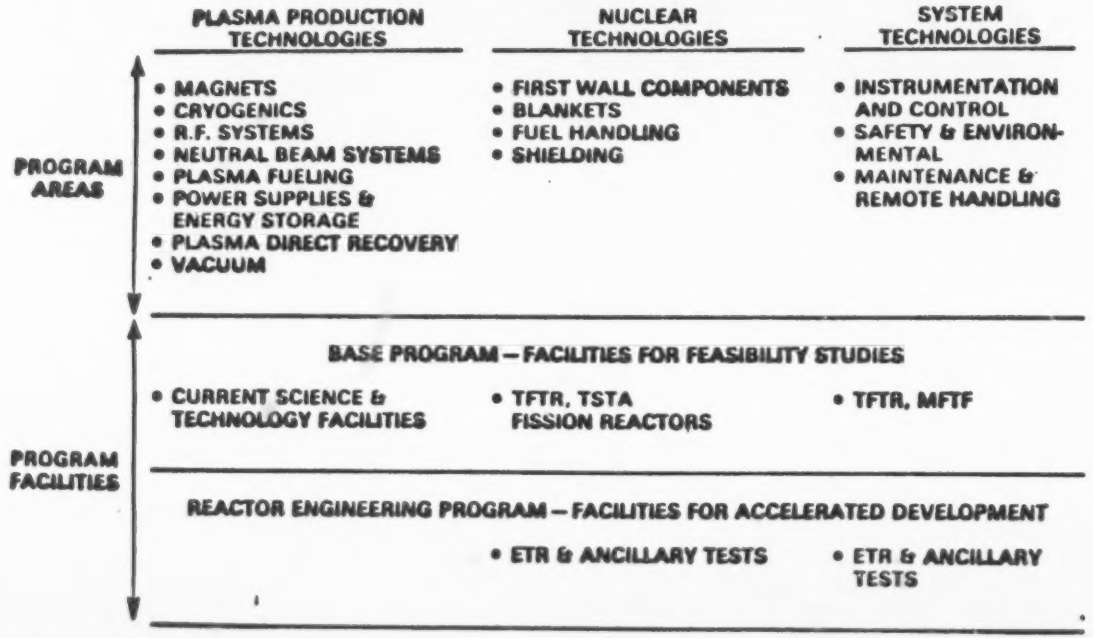
VI

THE EIGHT FUSION PHYSICS ISSUES MUST BE ADDRESSED AS A COMPLETE PLASMA SYSTEM



VII

THE THREE CLASSES OF FUSION TECHNOLOGIES CAN BE DEVELOPED INDIVIDUALLY TO A HIGH DEGREE, BUT MUST ULTIMATELY BE TESTED TOGETHER IN AN INTEGRATED SYSTEM



Q. 18. Please furnish for the record the percentage of total program funding associated with:

a) The National Laboratories (by laboratory)
b) Private Industry (by industry)
c) Universities (by university)
d) DOE Headquarters
e) Other Contractors

A. The percentages in the following table represent the distribution of Magnetic Fusion Energy Program funding by performer.

	FY 1982 Actual	FY 1983 Estimate
TOTAL National Laboratories[1]	82%	79%
Brookhaven National Laboratory	< 1%	< 1%
Idaho National Engineering Lab.	< 1%	< 1%
Monsanto	< 1%	
Pacific Northwest Laboratory	< 1%	< 1%
Sandia National Laboratory	1%	1%
Argonne National Laboratory	2%	1%
Lawrence Berkeley Laboratory	2%	2%
Hanford Engineering Development Lab.	4%	2%
Los Alamos National Laboratory	4%	4%
Lawrence Livermore National Lab.	20%	22%
Oak Ridge National Laboratory	20%	18%
Princeton Plasma Physics Lab.	28%	28%
Universities	9%	11%
Arizona University	< 1%	< 1%
Auburn University	< 1%	< 1%
California Institute of Technology	< 1%	< 1%
Columbia University	< 1%	< 1%
Connecticut, University of	< 1%	< 1%
Cornell University	< 1%	< 1%
Dartmouth College	< 1%	
Georgia Institute of Technology	< 1%	< 1%
Illinois, University of	< 1%	< 1%
Iowa University	< 1%	
John Hopkins University	< 1%	< 1%
La Jolla Institute	< 1%	
Maryland, University of	< 1%	< 1%
Michigan University	< 1%	< 1%
Missouri, University of	< 1%	< 1%
New York State University	< 1%	
New York University	< 1%	< 1%
North Texas State University	< 1%	< 1%
Northwestern University	< 1%	
Pennsylvania State University	< 1%	< 1%

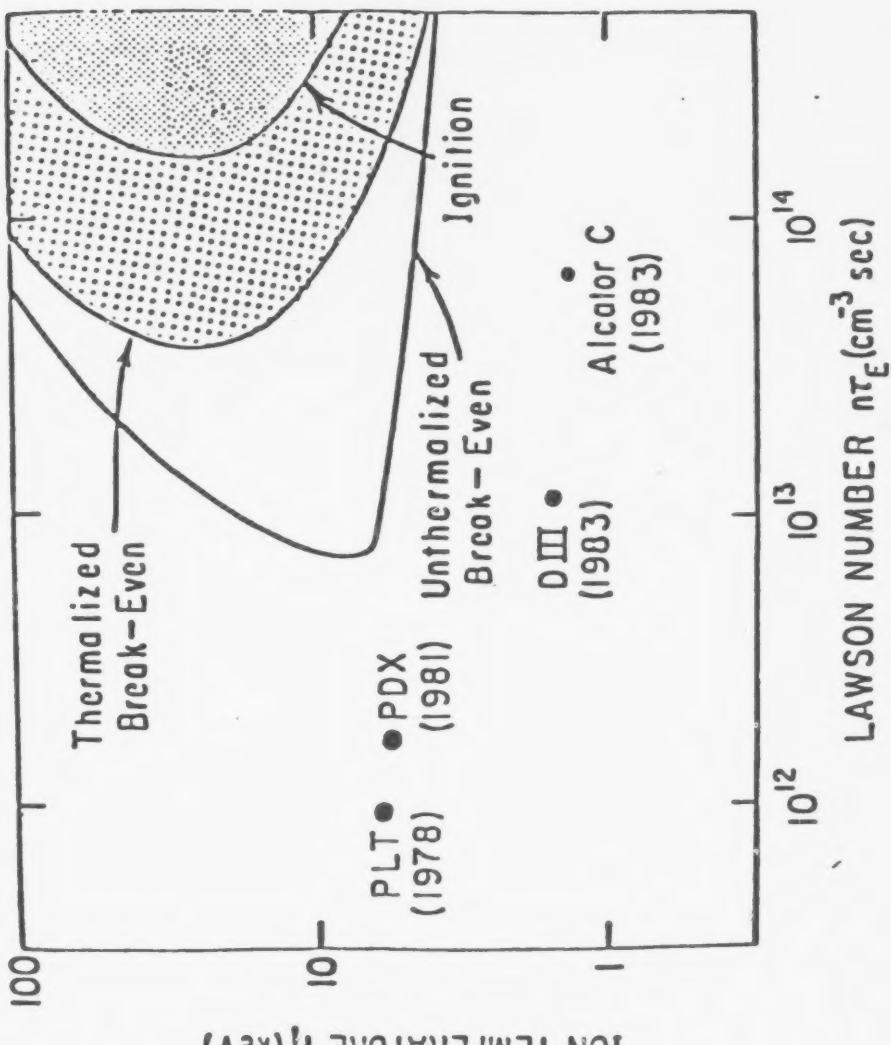
Rensselaer Polytechnical Institute	< 1%	< 1%
Rochester, University of	< 1%	
Smithsonian Institute	< 1%	< 1%
Stanford University	< 1%	
Oak Ridge Associated University	< 1%	< 1%
Virginia, University of	< 1%	
Washington, University of	< 1%	< 1%
Wesleyan University	< 1%	< 1%
Western Ontario University of	< 1%	< 1%
William and Mary College	< 1%	< 1%
Wisconsin, University of	< 1%	< 1%
Yale University	< 1%	< 1%
Colorado School of Mines		< 1%
Colorado University		< 1%
New York City, University of		< 1%
California, University of	1%	1%
Texas, University of	1%	1%
Massachusetts Institute of Tech.	4%	6%
Other Federal Agencies	1%	1%
Industry (direct only)	7%	8%
All Other	1%	1%

It should be noted that the industry line includes only those funds contracted directly to industry from Headquarters. A significant amount of funding included in the National Laboratory line is passed through the laboratory and subcontracted out to private industry for services and hardware. In FY 1983, for example, we estimate a total of 47% of the fusion budget was provided to industry. This 47% covers all private contracts, regardless of size, and includes over 15,000 companies. A listing of the major individual industrial participants in the FY 1982 budget year is included in the second question, which shows all contractors receiving over $100,000.

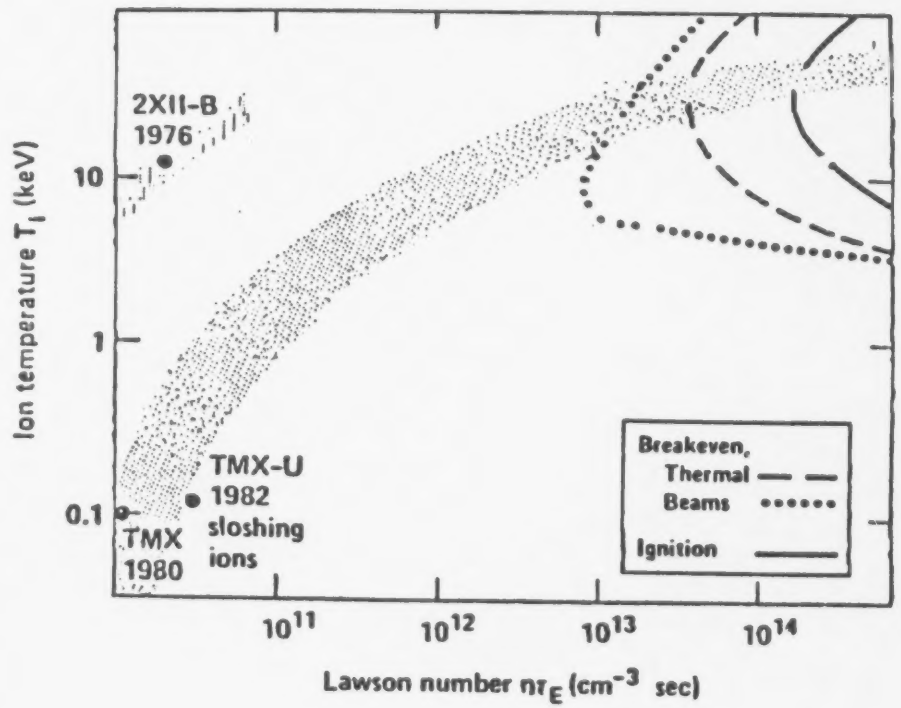
[1] This includes the Princeton Plasma Physics Laboratory which, while it is a university, is included as a national laboratory for the purposes of this crosscut.

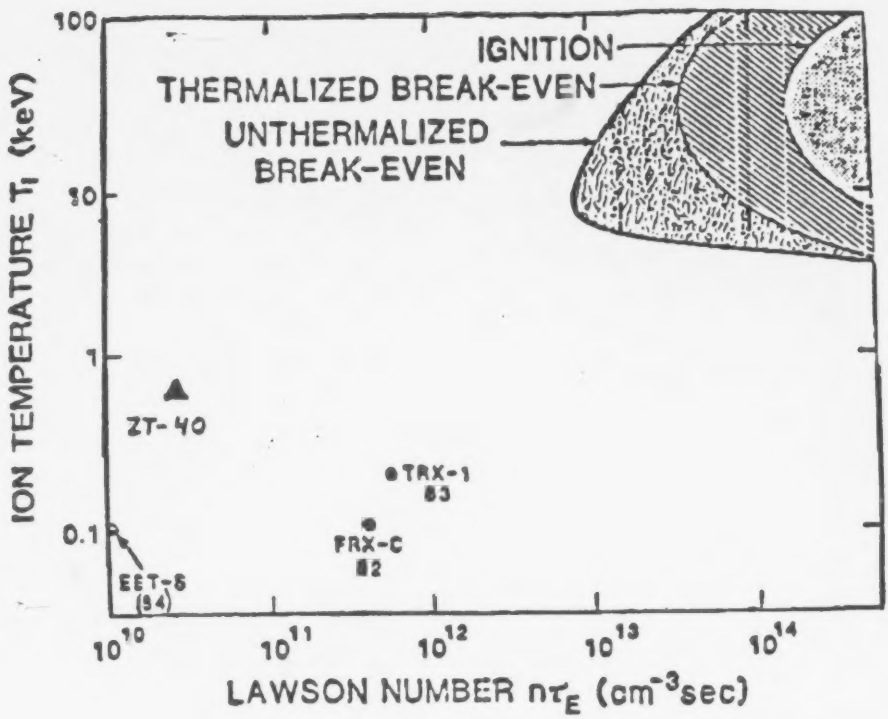
Q. 19. I have been told that the Lawson criterion cannot be applied to all fusion concepts and that certain concepts require individual curves (as discussed in the ORNL report by Nozawa and Steiner in 1973 and the LANL report by Freiwald and Gerwin in 1974). Do you then have separate data showing experiment status against the appropriate breakeven curves for each of the various concepts. If not, would you please provide them?

A. The ORNL Nozawa and Steiner paper and the LANL Freiwald and Gerwin paper dealt with engineering breakeven. Engineering breakeven is achieved when the power produced by the plasma is equal to the power supplied to all of the systems in a reactor required to heat and confine the plasma. Since engineering breakeven is subject to various system efficiencies, it differs for different fusion systems. For this reason the plasma breakeven, for which power deposited in the plasma is compared to power produced, has been adopted as a convenient way to evaluate progress. The Lawson Criterion is related to plasma breakeven. Since energy lifetime in the Lawson Criterion, τ_E reflects all of the energy losses in a plasma, the same breakeven curves plotting $n\tau_E$ vs. ion temperature, T_i, apply to all of the confinement geometries presently in use. Attached are these common graphs of $n\tau_E$ versus ion temperature with the achievements for tokamaks, mirrors, reversed field pinches and EBT indicated separately. These graphs contain profiles for both unthermalized breakeven where under heavy beam injection the beam produces a large number of fusion reactions, and thermalized breakeven where the reactions are produced in the bulk of the plasma. As can be seen from the tokamak graph, one of the important breakeven parameters, the Lawson criterion of $n\tau_E \geq 4 \times 10^{13}$ sec-cm^{-3} for thermalized breakeven, has been recently exceeded in Alcator C. The 1400-1500 eV plasma temperature associated with this $n\tau_E$ achieved is a factor of 3 lower than is required to move the plasma into the unthermalized breakeven regime.


ION TEMPERATURE T_i(keV)
100
10
1
10^{12}
10^{13}
10^{14}
LAWSON NUMBER $n\tau_E$(cm^{-3} sec)
Thermalized Break-Even
Ignition
Unthermalized Break-Even
PLT (1978)
PDX (1981)
DIII (1983)
Alcator C (1983)

PERFORMANCE OF TANDEM MIRRORS





VALUES OF THE CONFINEMENT PARAMETER $N\tau_E$ FOR VARIOUS ALTERNATE CONCEPT EXPERIMENTS.

FUSION POWER ASSOCIATES

2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879

(301) 258-0545

STEPHEN O. DEAN
PRESIDENT

November 30, 1983

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy
Research and Production
Committee on Science and Technology
Suite 2321 Rayburn House Office Building
Washington, D. C. 20515

Dear Ms. Lloyd:

I apologize for the delay in responding to your letter of November 1 requesting answers to questions concerning the DOE Comprehensive Program Management Plan for Magnetic Fusion. I appreciate the opportunity you provided me of testifying on the plan on October 20. My responses to your five questions are attached.

Sincerely,

Stephen O. Dean

Enclosure

cc: JD
RW

Responses to Questions for Dr. Stephen O. Dean

Question 1: Given the extreme physical conditions in a fusion reactor, what is your opinion of the potential for engineering solutions that might ultimately provide a reactor with a high availability factor?

The physical conditions in a fusion reactor are not so extreme that they will prevent the achievement of high availability for a fusion reactor. For example, although superconducting magnets operate at temperatures close to absolute zero, such magnets are routinely operated with high availability at many sites. Furthermore, although the temperature of the plasma desired is exceedingly high, we must remember that it is an extremely dilute gas and consequently the actual temperature experienced by the first wall of a fusion reactor is comparable to temperatures used today in many industrial processes. It is true that fusion reactors will bring together for the first time many advanced technologies. But the low availability experienced in the laboratory today is a result of the fact that these are experimental devices not designed for a high availability. Twenty years from now, when fusion becomes a reality, there is no reason to believe that the commercial product cannot be designed for high availability. Many complex technologies in commercial use today would have been considered unlikely to have a high availability twenty years ago and the same will be true in the 21st century.

Question 2: Do you believe increased international cooperation would accelerate or decelerate the pace of the U.S. Magnetic Fusion Program and why?

Increased international cooperation would accelerate the pace of the fusion program provided such cooperation is not used as an excuse to delay commitment to new facilities in the U.S. or elsewhere. I believe the U.S. should commit to those facilities it believes to be important and then to invite international cooperation in those programs rather than to delay commitment to the facilities pending international agreements to fund them.

Question 3: In his outline of "UCLA's Plasma and Fusion Program," Dr. John M. Dawson stated:

> "At present, with few exceptions, the national labs are wrapped up in their own programs and pay relatively little attention to or give much encouragement to university programs. They are mainly interested in efforts which will push what they are doing at the present time forward and show little interest in innovations which might impact their programs five or more years down the road. This attitude is understandable but for a long range program like the fusion program, such an extremely conservative approach is not beneficial."

I must respectfully disagree with the excerpt of Dr. Dawson's statement which you provided. While the national laboratories are understandably dedicated to successfully pursuing their mainline activities, each of the laboratories has shown its interest in innovation. For example, the mirror program as it

exists at LLNL today is quite different than it was several years ago due to a series of innovations in the concept. ORNL is pursuing an innovative stellarator concept which was absent in the U.S. program previously. The entire Los Alamos program is dedicated to innovative concepts and, in spite of their dedication to the tokamak, Princeton has an innovative compact concept experiment. Each of the laboratories has a variety of programs involving universities. Consequently, although university programs are an important source of new ideas and are much to be encouraged, it is by no means true that they are the only source of innovative ideas in the fusion program or that the laboratories are devoid of a long range view.

Question 4: Higher-density devices allow for shorter confinement time and thus smaller, more compact systems. What proportion of the program is directed towards alternative high-density concepts? Is the split between high density compacts and the main-line devices appropriate?

The appropriate question to ask relative to compact concepts is not what percent of the budget is devoted to them relative to the so-called mainline, but whether these concepts have adequate funding to pursue and prove out their most interesting features. I believe they do. Although it is certainly true that the compact concepts, as well as the mainline concepts, could profitably spend more money if more money were available to the fusion program as a whole, it must also be remembered that the compact concepts require less investment because of their early stage of development.

Question 5: What is your impression of the work being carried out in small fusion experiments and how should this type of effort be accommodated in the CPMP?

Good work is being done in small devices and the continuation of such work is essential and cost-effective. I believe that the Office of Fusion Energy at DOE is sensitive to the importance of small fusion experiments and does appropriately protect their funding relative to the larger programs.

NOV 28 1983

EBASCO SERVICES INCORPORATED

Two World Trade Center, New York, N.Y. 10048

EBASCO

Leonard F. C. Reichle
Group Vice President
Advanced Technology
and Special Projects

November 21, 1983

Honorable Marilyn Lloyd, Chai[illegible]
Subcommittee on Energy Researc[illegible]oduction
Committee on Science and Technology
Suite 2321, Rayburn House Office Building
Washington, D. C. 20515

Dear Chairman Lloyd:

This is in response to your letter dated November 1, 1983 regarding DOE's Comprehensive Program Management Plan for the Magnetic Fusion Program.

My response to your questions are set forth in Attachment A to this letter.

Best wishes!

Sincerely,

Len

LFCR:ia
Enclosure

Attachment A

Response to Questions

Ebasco Services Incorporated

1. What work is not being done on first wall materials? How adequately does the DOE's Comprehensive Program Management Plan address materials testing?

Effort is underway to quantify effects of 14 Mev neutrons. Particle and high heat fluxes are being addressed by TFTR, Doublet III and TEXTOR. Errosion/redeposition and morphology of the redeposited material need to be addressed.

If Bob Dowling's "Fusion Technology Development Plan" dated May 1983 is considered to be a part of the CPMP, the CPMP does address materials testing. The "Fusion Technology Development Plan" includes a first wall/blanket/shield program at ANL, but funding is not adequate to address critical materials issues in time to provide an engineering base for the TFCX.

2. What R&D is needed at this time to provide for development of superconducting coils which can perform better over longer time periods in a high neutron flux reactor? Has adequate shielding been developed? What role will MFTF magnets play in this work under the CPMP?

MFTF is not a D-T machine. Therefore, it can provide a data base on complex shape niobium-titanium magnets only. A development effort is needed for high-field niobium-tin magnets. Brookhaven and others are looking into this technology, but only for shapes and sizes applicable to accelerators. A shielding technology base seems to be "in-hand." Hybrid coils with copper inserts in superconducting coils for additional shielding should be looked at in more depth. Superconducting coil systems such as for TORE Supra in France should be followed since they have potential remote handling design features.

3. Are additional small to medium size facilities, such as those at MIT and the University of Texas, needed for experimental and theoretical work? Does the CPMP strategy provide for them?

In my opinion, no. We need to use existing medium size facilities to develop the physics base, then focus the program on large projects where these interdependent technologies are brought together to validate system performance.

4. Is there any fusion hardware that is not being developed at this time which should be under development in order to meet fusion energy objectives?

Remote handling hardware needs to be developed and demonstrated in the near term to ensure a viable data base for the next step in fusion i. e. TFCX. Although the CPMP calls for this base to come from TFTR, the present funding dictates only a modest initiative.

5. What are the primary differences between the ATF-1 and the OHTE (Ohmic Heating Toroidal Experiments-GA Technologies) fusion experiments?

ATF-1 is basically a stellerator where OHTE is basically a Reverse Field Pinch (RFP) that is driven by a stellerator coil. OHTE is a privately funded effort of GA Technologies and Phillips Petroleum. The proof-of-principle experiment at GA is encouraging.

6. We have been told that greater international cooperation in fusion energy development may not necessarily be in the best interest of the U. S. What is your opinion of the potential benefits and disadvantages?

Industrial nations are in competition with each other. We must not allow the United States to become dependent technologically on another nation, as we did in becoming dependent on the Mid-East for oil. We must not only maintain a viable fusion energy program, but one that seeks leadership. Otherwise, we will become dependent.

Foreign nations tend to negotiate exchange-of-technology agreements with us when our technology is more advanced than theirs. If ours lags, they tend to exclude the more advanced technology developed by them on the basis of "industrial proprietary information."

7. What is your opinion of the most appropriate budget levels for the TFCX? In particular, should it be in the FY 1985 program as a separate "line item?"

The Princeton Plasma Physics Laboratory estimates a TFCX cost (PACE) of $577-687 million for engineering, procurement and construction at a PPPL site and of $868-1,013 million at a new site. Using the higher estimate of $1.1 billion, and assuming an 8 year engineering-construction schedule, annual funding requirements (based on TFTR experience) might be the following:

Fiscal Years	85	86	87	88	89	90	91	92
Million $	100	200	200	150	150	100	100	100

Ebasco has not made an independent estimate of costs of the TFCX.

8. What benefits would the mirror program receive from the TFCX experience?

If TFCX develops niobium-tin superconducting magnets, this technology would benefit the mirror program. Examples of additional benefits are:

Steady state heat/particle removal

Advanced impurity control systems

Steady state operations

Steady state fueling; e. g. tritium pellet injector

Remote handling systems, design criteria, and remote handling equipment

9. What engineering work should be underway which is not now being accomplished?

An aggressive remote handling program needs to be initiated for TFTR to provide an engineering data base for TFCX. Also, niobium-tin magnets and radiation-hardened diagnostics (for example) should be developed further.

10 - Various program funding levels were included in a June 1983 paper presented by TRW. Are you familiar with this paper which shows a graph indicating funding escalating in 1985 and sloping up to a $1 billion level by 1992? If so, what is your opinion of this proposed doubling of the program in 7 years?

We are familiar with the TRW paper. The Magnetic Confinement Fusion Energy Program has achieved a highly productive momentum, which should be continued.

The next step is likely to cost about $1 billion. Most projects of this kind take approximately a decade to complete. The project cost and schedule are considered reasonable.

11 - What is the estimated cost of the TFCX including all construction, engineering, diagnostics equipment, etc.?

As stated above, PPPL estimates PACE costs as follows:

Site	Cost ($ Millions)
PPPL	577-687
New	868-1,013

Again, as noted above, Ebasco has not made an independent estimate of TFCX costs.

Plasma Fusion Center
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

December 30, 1983

Dr. Ronald E. Williams
Technical Consultant
Committee on Science
and Technology
U.S. House of Representatives
Suite 2321, Rayburn House
Office Building
Washington, D.C. 20515

Dear Dr. Williams:

As per your request, enclosed is my response to your written questions. If there are any further questions, don't hesitate to contact me.

Sincerely,

Ron Davidson

Ronald C. Davidson
Professor of Physics

RCD:gnc
Enclosure

QUESTIONS FOR DR. RON DAVIDSON

#1 • Is TFCX a too conservative step in magnetic fusion -- could not a more ambitious device yield bigger dividends sooner? What budget enhancement would be required for such an approach?

#2 • What work is being done on alternative fuels, such as protons on lithium or boron? What is the rationale for doing research on such fuels?

#3 • In the August 1983 MFAC Report on the long term role of universities in the Fusion Program, some faculty members had perceived concerns about government management, principally -

a) problems associated with stability of budgets;
b) the character of the DOE review procedure;
c) the need for better communication within the industry;
d) preventing conditions which encourage free expression and criticism.

Should any provisions be included in the Comprehensive Program Management Plan to address these concerns? What specific elements of the Fusion Program do you think can be most effectively performed at a university?

#4 • John M. Dawson, UCLA, noted in his outline of UCLA's Plasma and Fusion Program that "DOE tends to concentrate its attention on the efforts at national labs ----."; "that much of the good university research gets overlooked ----."; and "Many innovative ideas that could be of great value to the program are never funded because they are not of immediate applicability to one of the major devices." The CPMP identifies TFTR and MFTF operation as objectives. Would you comment on Dr. Dawson's statement in the context of the CPMP?

Ronald C. Davidson Response to Question #1

I fully support the findings and recommendations of the Magnetic Fusion Advisory Committee that the TFCX, which would demonstrate ignition and long-pulse equilibrium burn, should be the next major step in the fusion program. It would significantly advance technology development and the scientific data base for fusion, and would be a natural forerunner to subsequent major facilities. A budget increment in the range of 20-40% over a several year period would be required for TFCX. This is in contrast with the FED and the concomitant engineering development program described in the Fusion Engineering Act of 1980, which would require a program doubling over approximately seven years.

Ronald C. Davidson Response to Question #2

Research supported by the Department of Energy has been carried out at a seminal level on advanced fusion fuels for many years. The work at the University of Illinois, UCLA and Science Applications, Inc. is particularly noteworthy. Because of the low cross sections (relative to D-T), fusion reactors based on advanced fuels would operate at temperatures and confinement conditions well beyond the requirements for the first generation of D-T reactors. I view the research on advanced fuels as an important long-term investment. The first generation of commercial fusion reactors will operate with the less strenuous performance requirements of D-T fuel. In the longer term (perhaps 75-100 years), it is conceivable that "neutronless" fusion reactors using advanced fuels would be practical, although there remain many difficult questions related to economic attractiveness and low power density.

Ronald C. Davidson Response to Questions #3 and #4

The Magnetic Fusion Advisory Committee has carried out a detailed review of the long-term role of universities in the fusion program, and endorses the principal findings and recommendations of the panel chaired by Professor Gross of Columbia University. The Executive Summary of this report is attached for the record.

Historically, the universities have contributed to the national fusion program in several unique and important ways. These include: (a) the education and training of professional researchers; (b) providing the fusion program with a breadth of talent and intellect in the sciences and engineering; and (c) a major source of innovative ideas and scientific and technological advances. The universities have provided the fusion program with numerous innovative ideas and significant technical advances in a very cost-effective manner. The most effective contributions by the universities have been in the areas of small- and medium-scale experiments; plasma theory and computations; fusion technology development; and advanced concept development. I strongly believe that a continued strong component of university involvement with these characteristics is essential to a vigorous fusion research and development program for the forseeable future.

To increase DOE awareness of university contributions and concerns, and university awareness of DOE policy and budget issues and technical advances at the major laboratories and research centers, I would advocate an increased effort by all parties to improve and enhance communications. One constructive measure would be for the DOE Office of Fusion Energy to issue a monthly information newsletter that would keep the university community up-to-date on technical, policy, and programmatic matters.

To summarize, it is essential that the universities continue to play a major role in fusion energy development for the forseeable future. This should be a prominent element of DOE planning and policy.

MAGNETIC FUSION ADVISORY COMMITTEE

REPORT ON

EVALUATION OF THE LONG-TERM ROLE OF UNIVERSITIES IN THE FUSION PROGRAM

Submitted to

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C., 20585

August, 1983

R.C. Davidson, MIT; Chairman
H.K. Forsen, Bechtel; Vice Chairman
R.F. Beuligmann, GD
R.W. Conn, UCLA
J.E. Cummings, EPRI
T.K. Fowler, LLNL
H.P. Furth, PPPL
J.R. Gilleland, GA
R.A. Gross, Columbia U.
D.L. Kummer, MDAC
R.K. Linford, LANL
P. Murray, Westinghouse
M.N. Rosenbluth, U. Texas
M.W. Rosenthal, ORNL

August 3, 1983

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee met in San Francisco on August 2 and 3 and reviewed the findings and recommendations of the MFAC Panel evaluating the long-term role of universities in the fusion program (Panel No. 5). The Magnetic Fusion Advisory Committee endorses the principal findings and recommendations of the Panel (see attached summary and Panel Report) and makes the following ancillary comments and recommendations:

1. Historically, the universities have contributed to the national fusion program in several unique and important ways. These include: (a) the education and training of professional researchers; (b) providing the fusion program with a breadth of talent and intellect in the sciences and engineering; and (c) a major source of innovative ideas and scientific and technological advances. It is our assessment that a continued strong component of university involvement with these characteristics is essential to a vigorous fusion research and development program for the foreseeble future. In the view of MFAC and the Panel, the nature and overall present level of effort at universities is, on balance, about correct.

2. There are a growing number of cooperative programs between the universities and the national laboratories and to a much lesser extent between the universities and industry. Generally, this synergism has been very beneficial to the fusion program, and the Magnetic Fusion Advisory Committee strongly endorses a continuation of this trend in the future. In addition to collaborative efforts in the design and operation of experimental facilities, opportunities also exist for increased cooperation in training fusion engineering professionals in special subsystem and component areas.

3. As indicated in the Panel Report, a major role of the universities is in the area of training students and professional researchers. In this regard, the Magnetic Fusion Advisory Committee reinforces the Panel's finding of the present critical need for increasing numbers of trained experimentalists in the fusion program.

I look forward to discussing our findings and recommendations with you.

Sincerely,

Ronald C Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee

RCD:pt
Attachment

PRINCIPAL FINDINGS AND RECOMMENDATIONS

OF THE MAGNETIC FUSION ADVISORY COMMITTEE SUBPANEL

EVALUATING THE LONG-TERM ROLE OF UNIVERSITIES IN THE FUSION PROGRAM

July, 1983

Robert Borchers, Lawrence Livermore National Laboratory
Robert W. Conn, University of California, Los Angeles
Harry Dreicer, Los Alamos National Laboratory
Robert A. Gross (Chairman), Columbia University
Richard Hazeltine, University of Texas, Austin
Nicholas Krall, JAYCOR
Gerald Kulcinski, University of Wisconsin
Donald Kummer, McDonnell Douglas Astronautics Co.
John Landis, Stone & Webster Engineering Co.
Tihiro Ohkawa, G. A. Technologies
Ronald Parker, Massachusetts Institute of Technology
Fred Ribe, University of Washington
Dieter Sigmar, Oak Ridge National Laboratory
Thomas Stix, Princeton Plasma Physics Laboratory

EXECUTIVE SUMMARY

Findings:

In response to the charge given to this panel we have found that university contributions to the fusion program have been significant, extensive, and cost effective. University fusion research has had a major beneficial impact on the national program. We give examples in the body of our report and extensive documentation in the appendices.

We find that universities have several unique and important ways in which they contribute to the national fusion program. First and foremost, they educate new fusion professionals; second, universities provide to the fusion program an extraordinary breadth and diversity of talent in the sciences and in engineering; and finally, university fusion research programs historically have been, and must continue to be, a major source of discovery and innovation.

We believe that it is extremely important that university programs, focused upon fusion problems of high intellectual challenge, continue to be supported. We find that the present fraction of the national program that supports university fusion research is reasonable and provides a sensible university base program. We believe that fusion is extremely important to our nation's future, and the universities must be asked to give their very best to this program. Because of the great challenge and the great promise of fusion energy, the very best people, wherever they are found, must be engaged in our national program.

The role of universities in the United States fusion program must be considered in the context of the entire national effort. There are four complementary areas of activity: plasma research, fusion-technology, hardware development and construction, and the education and training of personnel. Performing these activities are the national laboratories, the universities, and industry. These three groups complement each other. The resources to attack the fusion problem are: personnel, existing laboratory facilities, and the technical capabilities of the nation. The challenge is to optimize the use of these resources. This optimization must take into account constraints such as: (a) funds limit the number of major paths that can be followed; and (b) machine time and accessibility limit the number of senior scientists that can usefully contribute to the operation of a single experiment. We believe that a number of critical issues can be successfully addressed on small- and medium-scale devices. We also note that communication between

scientists and engineers at different laboratories and institutions is rapid and intense. Optimization of resources will inevitably comprise an entire spectrum of levels of activity. It is within the context of this entire complex that universities, with professors, ambitious and bright graduate students, and post-doctoral associates, make their unique and prolific contributions.

In order to resolve issues critical to the development of fusion power, researchers in the national program in general and the university component in particular will require access to a spectrum of experimental devices, ranging in size from the relatively small devices typical of many existing university research programs to the very large facilities now found in the national labs. The former class of experiments have been and will continue to be useful in isolating and studying basic plasma phenomena, while the latter have the unique capability to create plasma with near-fusion parameters.

As the plasmas produced in the large devices become more fusion relevant, the need to study basic phenomena with more advanced plasma parameters will also grow. Consequently, it will be necessary to build and operate a number of intermediate or "medium-scale" experiments whose primary functions will be to explore basic plasma behavior at substantially more advanced parameters than are possible with the present university-scale devices. In this context, medium scale refers to devices which will probably cost in the range of $3-30M. It is also clear that many of these devices should be sited at those universities where research competence and the capability to operate moderately large experiments exist.

The U. S. magnetic fusion project has, from nearly its beginning, contained research programs located at universities. Financial support for their operation is today at about fifty-one million dollars per year, which is about eleven percent of the total fusion operating budget. There are seven university programs which receive support ranging from one to twenty-two million dollars per year each, for a total of about forty-five million dollars per year. Twenty-nine universities have smaller programs whose total funding is about six million dollars per year.

University programs are the major source of new fusion scientists and engineers; between 70 to 80 new Ph.D.'s per year are trained. This approximately meets the present needs. In Appendix 5 we give the yearly production of fusion Ph.D.'s from universities that are supported by the fusion program, and we list the employers for these graduates. Although there is at present a reasonable balance between need and newly trained professionals, it is currently difficult to find new well-trained experimentalists. Competition from the Department of Defense, and some parts of the private sector, for persons trained by university fusion programs, is increasing.

Universities have made substantial contributions in both theory and experiments for the fusion program. For example, university scientists and engineers have contributed significantly in proposing,

exploring, and developing confinement scaling laws, impurity control techniques, sloshing ions for tandem mirrors, second stability regime physics for tokamaks, pumped limiters, special diagnostics, and current drive techniques. University fusion research has been innovative in proposing new magnetic confinement concepts. University-based programs have made major contributions to fusion technology by pioneering early reactor designs, suggesting new materials for use in fusion reactors, developing new blanket designs, and exploring the possibilities for advanced fusion fuels.

There are problems that some faculty in the university fusion community have perceived with DOE management of the program and there are serious concerns about the future. Some of the more widely held concerns that the university fusion community has transmitted to the panel involve:

a) the problems associated with budget changes and the desire for stability of university basic research programs; Fiscal stability is extremely important because of the need for continuity of graduate student support;

b) the character of the DOE review procedure. The criteria used in judging proposals should emphasize excellence, merit, and capability;

c) the need for better communication with the entire university fusion community and the dissemination of information;

d) the uncertainty of future manpower needs for the fusion program;

e) the management of small university programs. DOE should be flexible in its management approach to university programs so as to allow for the conception and development of innovative ideas;

f) the need to preserve the conditions which encourage free expression and criticism.

We have also examined how universities are used in the Japanese and European fusion programs. Universities have a relatively large role in the Japanese program and there is close collaboration with industry. In Europe, university involvement is modest, but where it exists there is close collaboration with Euratom laboratories. It is difficult to draw precise comparisons with the U. S. fusion program because of different funding methods, etc. Details about the Japanese and European university fusion programs are given in the appendix.

Recommendations:

The Panel recommends that a strong and vigorous university fusion program be continued. Specific recommendations are:

1. The unique capabilities of the universities in carrying out research for the fusion program and in training professionals for the fusion program should be utilized to the fullest. Adequate funding for this purpose must be a basic component of the fusion budget.

2. To continue to attract the best minds, the national fusion program must involve universities in fusion's most challenging research and development.

3. The magnetic fusion energy program should use universities in all aspects of the program. Some universities can undertake relatively large-scale research programs. Others can make effective contributions with smaller programs, including the single investigator.

4. There are problems in the fusion program which are best addressed with medium-sized experimental facilities. Consistent with recommendations 1. and 2., some of these facilities should be sited at universities. Joint university-industry or national lab teams could be used to assist in the design, construction, and operation of these new experiments.

5. Plasma research in both theory and experiments needs to be sustained and encouraged. A core effort in theoretical and experimental non-programmatic basic research needs to be carried out because not enough is known about the physics of plasmas. Funds should be consciously dedicated to continue support of this basic plasma research. This support should be directed to problems of high intellectual challenge, some of which can best be solved at universities.

6. Research and development of fusion technology needs to be encouraged and expanded at universities where special engineering expertise exists. It is particularly important that some fusion engineering experiments be located at universities to attract bright new engineering students.

7. We find that the present fraction of the national program that supports university fusion research is reasonable and provides a sensible university base program. New medium-size experiments as recommended will require incremental funding above the base program.

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8. There is need for more complete and timely communication between the DOE Office of Fusion Energy and all university fusion groups. A monthly newsletter should be published for the entire fusion program. It should include information about program changes, review panels, scientific and technical progress, personnel, etc.

9. Personnel exchanges between universities, national laboratories, and industry should be enhanced.

10. University fusion programs have a rather different character and purpose than those programs in national laboratories and industry. They explore new ideas and seek to understand the basic underlying science and technology. Flexibility is essential for university research and development programs. The Office of Fusion Energy is encouraged to manage these programs with that spirit and understanding.

11. Every effort must be made to provide stable and continuous funding for university programs. This is particularly important for smaller-sized programs, where longer range funding commitments are encouraged. Continuity of financial support is vital for graduate students.

12. The magnetic fusion energy technology fellowships are valuable for attracting the best talent into fusion. Similar fellowships should be initiated for Applied Plasma Physics and Confinement.

13. The universities are an important source of independent views and objective criticism. It is important to preserve conditions that encourage such free expression.

CHARGE TO

MAGNETIC FUSION ADVISORY COMMITTEE

EVALUATING THE LONG-TERM ROLE OF

UNIVERSITIES IN THE FUSION PROGRAM

Department of Energy
Washington, D.C. 20585

January 6, 1983

Dr. Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee
Director, Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street, NW16-202
Cambridge, MA 02139

Dear Ron,

I have read the Report on Evaluation of Magnetic Confinement Concepts submitted to the Department on November 1, 1982, by the Magnetic Fusion Advisory Committee (MFAC). The Report assisted us in developing budgets for the coming year. Thank you for your efforts in producing a fine piece of work.

Now, I would like your advice in two additional areas. The first is on the role of MFTF-B, and the second is on the role of universities in the fusion program.

In my June 1, 1982 letter to the Committee, I requested MFAC to consider programmatic roles for the TFTR, following feasibility experiments. It is also important that the other major facility, MFTF-B, be utilized to the greatest benefit of the fusion program. Therefore, as an extention of my charge #3 to MFAC last June, I would like the Committee to consider possible programmatic roles for MFTF-B following the completion of its initial experimental program.

In your deliberations, you should take into consideration the possibility of technology-oriented roles for MFTF-B, as well as physics-oriented roles. Facilities upgrade modifications should be included in your deliberations, in analogy with the charge on TFTR.

I would like your final report on this topic by April 1983.

To assist the Department in making effective use of its university-based fusion research program, I would like MFAC to advise us on the long-term role of universities in the fusion program.

Fusion research involves the triad of universities, national laboratories, and industry. Because of the evolutionary nature of the fusion program, the role of each of them will likely change. Therefore, emphasis should be put on exploring new forms of participation, rather than solely changing the level. The Office of Fusion Energy has the responsibility to ensure that these changes strengthen the overall program.

At present, universities are generally involved with smaller scale exploratory research. However, the approach toward reactor conditions tends to imply larger and more costly physics and engineering experiments, as well as more focused and detailed reactor studies. Therefore, these differences must be taken into account as universities review their programs.

Considering the evolving nature of the fusion program, what could the Office of Fusion Energy do, and what could universities do to maintain strong and productive university-based research?

Furthermore, the magnetic fusion energy program, will need a continuing supply of creative, appropriately trained scientists and engineers. A recent manpower study performed for the Office of Fusion Energy indicates that sufficient numbers of technical personnel are likely to be available in the coming decades, for certain specific technical disciplines. However, fusion requires the application of general technical expertise to specific fusion problems as well as certain specialized skills in these areas. What actions could the Office of Fusion Energy take now to be certain that universities will, in fact, be able to produce the types and quality of personnel needed by the fusion program in the future?

The Energy Research Advisory Board (ERAB) has been asked to examine the broad issue of universities' research and development activities, and to address specifically, the effectiveness of the Department's policies and working relationships with the academic community. A copy of the Deputy Secretary's charge letter to ERAB is enclosed.

I would like your final report on this topic by July 1983.

I consider these to be very significant issues for the Department and I look forward to receiving the advice of your Committee.

Sincerely,

A. Trivelpiece

Alvin W. Trivelpiece
Director, Office of
Energy Research

Enclosure

EBASCO SERVICES INCORPORATED EBASCO
Two World Trade Center, New York, N.Y. 10048

Leonard F. C. Reichle
Group Vice President
Advanced Technology
and Special Projects

November 21, 1983

Honorable Marilyn Lloyd, Chairman
Subcommittee on Energy Research & Production
Committee on Science and Technology
Suite 2321, Rayburn House Office Building
Washington, D. C. 20515

Dear Chairman Lloyd:

This is in response to your letter dated November 1, 1983 regarding DOE's Comprehensive Program Management Plan for the Magnetic Fusion Program.

My response to your questions are set forth in Attachment A to this letter.

Best wishes!

Sincerely,

Len

LFCR:ia
Enclosure

Attachment A

Response to Questions

Ebasco Services Incorporated

1. What work is not being done on first wall materials? How adequately does the DOE's Comprehensive Program Management Plan address materials testing?

Effort is underway to quantify effects of 14 Mev neutrons. Particle and high heat fluxes are being addressed by TFTR, Doublet III and TEXTOR. Errosion/redeposition and morphology of the redeposited material need to be addressed.

If Bob Dowling's "Fusion Technology Development Plan" dated May 1983 is considered to be a part of the CPMP, the CPMP does address materials testing. The "Fusion Technology Development Plan" includes a first wall/blanket/shield program at ANL, but funding is not adequate to address critical materials issues in time to provide an engineering base for the TFCX.

2. What R&D is needed at this time to provide for development of superconducting coils which can perform better over longer time periods in a high neutron flux reactor? Has adequate shielding been developed? What role will MFTF magnets play in this work under the CPMP?

MFTF is not a D-T machine. Therefore, it can provide a data base on complex shape niobium-titanium magnets only. A development effort is needed for high-field niobium-tin magnets. Brookhaven and others are looking into this technology, but only for shapes and sizes applicable to accelerators. A shielding technology base seems to be "in-hand." Hybrid coils with copper inserts in superconducting coils for additional shielding should be looked at in more depth. Superconducting coil systems such as for TORE Supra in France should be followed since they have potential remote handling design features.

3. Are additional small to medium size facilities, such as those at MIT and the University of Texas, needed for experimental and theoretical work? Does the CPMP strategy provide for them?

In my opinion, no. We need to use existing medium size facilities to develop the physics base, then focus the program on large projects where these interdependent technologies are brought together to validate system performance.

4. Is there any fusion hardware that is not being developed at this time which should be under development in order to meet fusion energy objectives?

Remote handling hardware needs to be developed and demonstrated in the near term to ensure a viable data base for the next step in fusion i. e. TFCX. Although the CPMP calls for this base to come from TFTR, the present funding dictates only a modest initiative.

5. What are the primary differences between the ATF-1 and the OHTE (Ohmic Heating Toroidal Experiments-GA Technologies) fusion experiments?

ATF-1 is basically a stellerator where OHTE is basically a Reverse Field Pinch (RFP) that is driven by a stellerator coil. OHTE is a privately funded effort of GA Technologies and Phillips Petroleum. The proof-of-principle experiment at GA is encouraging.

6. We have been told that greater international cooperation in fusion energy development may not necessarily be in the best interest of the U. S. What is your opinion of the potential benefits and disadvantages?

Industrial nations are in competition with each other. We must not allow the United States to become dependent technologically on another nation, as we did in becoming dependent on the Mid-East for oil. We must not only maintain a viable fusion energy program, but one that seeks leadership. Otherwise, we will become dependent.

Foreign nations tend to negotiate exchange-of-technology agreements with us when our technology is more advanced than theirs. If ours lags, they tend to exclude the more advanced technology developed by them on the basis of "industrial proprietary information."

7. What is your opinion of the most appropriate budget levels for the TFCX? In particular, should it be in the FY 1985 program as a separate "line item?"

The Princeton Plasma Physics Laboratory estimates a TFCX cost (PACE) of $577-687 million for engineering, procurement and construction at a PPPL site and of $868-1,013 million at a new site. Using the higher estimate of $1.1 billion, and assuming an 8 year engineering-construction schedule, annual funding requirements (based on TFTR experience) might be the following:

Fiscal Years	85	86	87	88	89	90	91	92
Million $	100	200	200	150	150	100	100	100

Ebasco has not made an independent estimate of costs of the TFCX.

8. What benefits would the mirror program receive from the TFCX experience?

If TFCX develops niobium-tin superconducting magnets, this technology would benefit the mirror program. Examples of additional benefits are:

Steady state heat/particle removal

Advanced impurity control systems

Steady state operations

Steady state fueling; e. g. tritium pellet injector

Remote handling systems, design criteria, and remote handling equipment

9. What engineering work should be underway which is not now being accomplished?

An aggressive remote handling program needs to be initiated for TFTR to provide an engineering data base for TFCX. Also, niobium-tin magnets and radiation-hardened diagnostics (for example) should be developed further.

10 - Various program funding levels were included in a June 1983 paper presented by TRW. Are you familiar with this paper which shows a graph indicating funding escalating in 1985 and sloping up to a $1 billion level by 1992? If so, what is your opinion of this proposed doubling of the program in 7 years?

We are familiar with the TRW paper. The Magnetic Confinement Fusion Energy Program has achieved a highly productive momentum, which should be continued.

The next step is likely to cost about $1 billion. Most projects of this kind take approximately a decade to complete. The project cost and schedule are considered reasonable.

11 - What is the estimated cost of the TFCX including all construction, engineering, diagnostics equipment, etc.?

As stated above, PPPL estimates PACE costs as follows:

Site	Cost ($ Millions)
PPPL	577-687
New	868-1,013

Again, as noted above, Ebasco has not made an independent estimate of TFCX costs.

FUSION POWER ASSOCIATES

2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879

(301) 258-0545

STEPHEN O. DEAN
PRESIDENT

November 30, 1983

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy
Research and Production
Committee on Science and Technology
Suite 2321 Rayburn House Office Building
Washington, D. C. 20515

Dear Ms. Lloyd:

I apologize for the delay in responding to your letter of November 1 requesting answers to questions concerning the DOE Comprehensive Program Management Plan for Magnetic Fusion. I appreciate the opportunity you provided me of testifying on the plan on October 20. My responses to your five questions are attached.

Sincerely,

Stephen O. Dean

Enclosure

Responses to Questions for Dr. Stephen O. Dean

Question 1: Given the extreme physical conditions in a fusion reactor, what is your opinion of the potential for engineering solutions that might ultimately provide a reactor with a high availability factor?

The physical conditions in a fusion reactor are not so extreme that they will prevent the achievement of high availability for a fusion reactor. For example, although superconducting magnets operate at temperatures close to absolute zero, such magnets are routinely operated with high availability at many sites. Furthermore, although the temperature of the plasma desired is exceedingly high, we must remember that it is an extremely dilute gas and consequently the actual temperature experienced by the first wall of a fusion reactor is comparable to temperatures used today in many industrial processes. It is true that fusion reactors will bring together for the first time many advanced technologies. But the low availability experienced in the laboratory today is a result of the fact that these are experimental devices not designed for a high availability. Twenty years from now, when fusion becomes a reality, there is no reason to believe that the commercial product cannot be designed for high availability. Many complex technologies in commercial use today would have been considered unlikely to have a high availability twenty years ago and the same will be true in the 21st century.

Question 2: Do you believe increased international cooperation would accelerate or decelerate the pace of the U.S. Magnetic Fusion Program and why?

Increased international cooperation would accelerate the pace of the fusion program provided such cooperation is not used as an excuse to delay commitment to new facilities in the U.S. or elsewhere. I believe the U.S. should commit to those facilities it believes to be important and then to invite international cooperation in those programs rather than to delay commitment to the facilities pending international agreements to fund them.

Question 3: In his outline of "UCLA's Plasma and Fusion Program," Dr. John M. Dawson stated:

> "At present, with few exceptions, the national labs are wrapped up in their own programs and pay relatively little attention to or give much encouragement to university programs. They are mainly interested in efforts which will push what they are doing at the present time forward and show little interest in innovations which might impact their programs five or more years down the road. This attitude is understandable but for a long range program like the fusion program, such an extremely conservative approach is not beneficial."

I must respectfully disagree with the excerpt of Dr. Dawson's statement which you provided. While the national laboratories are understandably dedicated to successfully pursuing their mainline activities, each of the laboratories has shown its interest in innovation. For example, the mirror program as it

exists at LLNL today is quite different than it was several years ago due to a series of innovations in the concept. ORNL is pursuing an innovative stellarator concept which was absent in the U.S. program previously. The entire Los Alamos program is dedicated to innovative concepts and, in spite of their dedication to the tokamak, Princeton has an innovative compact concept experiment. Each of the laboratories has a variety of programs involving universities. Consequently, although university programs are an important source of new ideas and are much to be encouraged, it is by no means true that they are the only source of innovative ideas in the fusion program or that the laboratories are devoid of a long range view.

Question 4: Higher-density devices allow for shorter confinement time and thus smaller, more compact systems. What proportion of the program is directed towards alternative high-density concepts? Is the split between high density compacts and the main-line devices appropriate?

The appropriate question to ask relative to compact concepts is not what percent of the budget is devoted to them relative to the so-called mainline, but whether these concepts have adequate funding to pursue and prove out their most interesting features. I believe they do. Although it is certainly true that the compact concepts, as well as the mainline concepts, could profitably spend more money if more money were available to the fusion program as a whole, it must also be remembered that the compact concepts require less investment because of their early stage of development.

Question 5: What is your impression of the work being carried out in small fusion experiments and how should this type of effort be accommodated in the CPMP?

Good work is being done in small devices and the continuation of such work is essential and cost-effective. I believe that the Office of Fusion Energy at DOE is sensitive to the importance of small fusion experiments and does appropriately protect their funding relative to the larger programs.

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